THE JOURNAL

OF THE

RÖNTGEN SOCIETY.

EDITED BY

J. H. GARDINER, F.C.S.,

WITH THE CO-OPERATION OF THE COUNCIL.

VOLUME XII

1915-16.

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THE JOURNAL OF THE RÖNTGEN SOCIETY.

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GENERAL MEETING.

Session 1915-1916.

The new session opened on November 2nd with a meeting at the Institution of Electrical Engineers, Victoria Embankment, W.C.

On account of the pressure of his official duties, the retiring President, Sir Alfred Pearce Gould, was not able to be present and the chair was taken by our Past President W. Duddell, F.R.S.

In the absence of the retiring President the vote of thanks, which the Society desired to accord to him, had to remain unexpressed, but the Council and the Society desire to take this opportunity of tendering their sincere appreciation and thanks to him for the thoroughness with which he carried out those duties that he so generously undertook, full knowing the great strain that his professional work, due to the present distressful war, would lay upon him. The success of the past session was largely due to his leadership, and we look forward to the time when the war will be a thing of the past and Sir Alfred will be able to devote his full energies to the investigation and application in clinical practice of those forms of radiations with which the Society deals.

Mr. Duddell announced that since the last meeting the Council had under consideration the formation of a set of rules or precautions to be taken by X-ray workers to guard them against the dangers to which they are subject. (This work has been completed and the recommendations, printed upon a card suitable for hanging upon the wall, together with an explanatory letter, is enclosed with this issue of the JOURNAL.)

On account of the lateness of the hour, it was proposed that the Nominations and other official business of the evening should be "taken as read," and after a few introductory remarks with reference to the new President, Mr. Duddell vacated the chair, which was taken by Mr. J. H. Gardiner, who delivered the following Presidential address:—

PRESIDENT'S ADDRESS.

The session we are about to enter upon is the second, and I sincerely hope that it will be the last, when our work will have to go on under the trying conditions of one of the most terrible and senseless wars that will have to be recorded in human history. I feel sure that no sober-minded person can regard it in any other light. I have by me a post card that I received in the early weeks of the war from a man who is now presumably one of

our enemies in which it is referred to as "that atrocious" war; but in all the atrocity, in all the misery attendant upon the present terrible condition of things, we of the Röntgen Society have the consciousness that the larger part of our activity is directly concerned in mitigating to no small extent the horrors immediately following upon the present conflict.

In this connection I hope you will bear with me for the moment if I suggest that, while on the one hand there has of late years been great developments in the inventions of devices for human destruction, on the other there has proceeded the discovery, in many cases accidentally, of re-actions, properties and effects that are used to relieve in very large measure the suffering and distress resulting from the employment of those inventions. One shrinks from the thought of what would be the state of things just now if we were ignorant of the power of anæsthetics, the value of antiseptics, or the properties of X-rays.

It is often the practice upon occasions like the present to look back and review the past history of our Society, and although I think that I might be able to do this pretty fully, for I have attended nearly every meeting that has been held since its foundation, yet I do not propose to do so except very briefly indeed.

When, in 1896, the discovery was made of those novel radiations having the remarkable properties with which we are now familiar, their importance in surgery was immediately recognised and it was largely on that account that this Society came into existence.

I can confidently assert that it has fully justified its existence; it has formed the debating-ground for members both of the medical and purely physical professions where we have met to our mutual advantage; the attention of physicists has been directed to properties and re-actions due to the radiations that otherwise they would in large measure have been ignorant about; medical applications of electricity have been placed upon the sound basis of practical science, and those *physical* members whose business it is to devise and manufacture the complicated apparatus necessary for the practice of radiology have been enabled to keep in close touch with the exact requirements of both medical and physical professions, with the result that they have been able to produce the magnificent equipment that is now to be found in every hospital and laboratory of importance.

Looking back over the past twenty years one can recall the weird feelings with which the laity at least regarded the radiographs showing the bony structure of the hand and wrist that were first exhibited in this country by our past-President, Mr. A. A. Campbell Swinton, and remember the rush by the great class of scientific amateurs to reproduce the latest new thing; great was the demand for induction coils, for batteries, photographic apparatus, and above all for the Crooke's tubes with which to produce these wonderful photographs. This last item was the most serious of all for they were very fragile, generally breaking down after a first experiment or even before. Soon the stock of these scientific novelties was exhausted and the few of us who were able to construct them were kept busy to supply our own needs and the demands of our friends.

The excitement was great, the scientific press was crowded with discoveries and announcements and every one was talking about the "new light" but it was soon over, the merely curious turned to other things, and from all the hubbub there emerged the Röntgen Society, composed chiefly of men who saw in the new discovery the prospect of serious and useful *work*.

The first real advance was in the application of the so-called focus tube for the production of X-rays by our past-President,

Mr. Herbert Jackson, and by the demonstration of the origin and physics of the X-ray by Professor A. W. Porter, Mr. Campbell Swinton, and others, radiography lost most of the uncertainty and disappointment that marked the earlier days, and really serious work was being done both in the laboratory and in the hospitals. Just at this period the brilliant work of Sir James Mackenzie Davidson laid the foundation upon which all the more recent developments of localisation are built.

It is a curious thing, but it often happens, that nature appears to resent an intrusion into her secrets, and will sometimes make the intruder pay dearly. It was so in the case of X-rays; not only was that beneficent provision that we call pain (which tells us that something is wrong if there is time to remedy it) withheld, but the harm that was being done gave no warning, and thus was continued until after some weeks' interval the result of the accumulated indiscretions became apparent.

I will not pursue this unhappy subject further; enough to say that the most active and earnest of our workers were the worst victims, and the result was seen in empty chairs at our Councils and in the vanishing of familiar figures at our meetings. All honour to their memory. In most cases they gave their best ungrudgingly for the good of their fellow men.

Happily now the worst of the dangers attending the use of X-rays is known and can in a large measure be guarded against, but caution is still needed, and I for one am not prepared to endorse an opinion that I have heard expressed here more than once, that "the man who burns his patient is a criminal, and he who burns himself is a fool." The recent experiences of "late X-ray re-actions," recorded by Dr. Finzi and others, after the employment of all known precautions, are in themselves sufficient to warn us that we do not yet know all about the effects due to X-rays.

At the present time installations where X-rays are being used are multiplying rapidly, and the work has of necessity to be done by those whose past experience is slight or is lacking altogether. In view of the serious results that may follow unless due care is taken, the Council has, in the name of the Society, devised a set of suggestions for the protection of X-ray workers; these will be supplied to all members and to as many public institutions as possible.

It is hoped that if attention is paid to the warnings given much trouble may be avoided.

But the discovery of the destructive effects of X-rays, serious though they were, had its brighter side, for the therapeutic value of irradiation was at once recognised, and the science of radio-therapy which has resulted from it already rivals that of radiography pure and simple. Much indeed has been learned, much has been and is still being done. While we deplore the disasters of the earlier days we can record with satisfaction that one more weapon has been added to the armoury of the physician in the conflict he is ever waging with disease and death. I do not propose occupying more of your time with the discussion of the result of the past 20 years' acquaintance with X-rays and their allied radiations, I shall do no more than refer to some of the numerous applications.

In surgery, as we all know, they have become invaluable; in dentistry they are rapidly becoming a necessity. We have often been shown here how many diseases can be detected and remedied in their early stages to the immense saving of that pain that carries so little sympathy.

X-rays have been largely used in Zoological research as a means of demonstrating the osteological structure of small reptiles and fishes in a way impossible by any other means; in palæontology for the discovery of fossils in rocks; in engineering for the detection of flaws in metals and blow holes in

castings; as a means of distinguishing between real and artificial gems; for the examination of sealed packets; for the examination of pearl oysters for the detection of pearls; and numerous uses which are extending continually.

But I will not weary you with history, which after all is a poor thing to live upon; the past is past, it is the present that really matters, for it is a truism that our use of the present determines our part in the future, and I feel sure that at this particular period the present is the thing that matters to us.

If the Röntgen Society is to continue and extend its usefulness, that vitality which is the essence of our existence must not be allowed to wane, even in the times of strain in which we find ourselves; what I refer to as the vitality of our Society may be crystallised into one word and that word is *research*. Science is inexhaustible and our actual knowledge of the things around us is exceedingly superficial; the energy of every individual member should be bent upon the task of extending in some little measure the bounds of the particular sphere that happens to be under his hand, no matter how trivial or unimportant it may appear, for it is often the apparently unimportant things that lead to great results. There is no pursuit so fascinating or that carries with it greater pleasure than that of discovering one thread more in the pattern of that wonderful design that we call natural phenomena.

It has been my good fortune for many years to be in a position to observe intimately the beginnings of many of these discoveries upon which our science has been built, and what has struck me most forcibly is the apparently unimportant and often accidental circumstance that led to those advances in knowledge which are now our accepted facts. Many things have been brought to my notice that are perhaps not generally known and I think that it may not be out of place if I refer to a few of those that more directly concern the interests of our Society, for the circumstances that I have in my mind show very clearly that many of the great scientific achievements with which we are now familiar and with which the world could not possibly do without, are the outcome of a simple desire upon the part of one or another to search after truth for the truth's sake.

One of the earliest things that I remember was in 1875, shortly after the invention of the telephone by Graham Bell, Professor Hughes set the scientific world on fire by the announcement of the microphone or sound magnifier. I well recollect how the professor exhibited his ingeniously constructed little machine to the members of the Royal Society. Second only to the interest of his discovery was the fact that there was no complicated or costly apparatus involved. He prefaced his communication with the following remark :—

"All the experiments detailed in this paper were made with the simplest possible means, no apparatus of any kind constructed by a scientific instrument maker was employed"—and the whole of his apparatus was made out of cigar boxes, bits of charcoal, copper wire and sealing wax, even the telephone was a home-made affair consisting of a small bar magnet, a coil of wire, and a square of thin sheet iron clamped between two pieces of wood.

With these simple contrivances Hughes found it possible to magnify the faintest sounds: the tick of a watch, the movement of a feather, and the footsteps of a fly could be made audible at almost any distance.

I have here one of the machines of the kind that Hughes showed, and connected with it a telephone that I made in those far off days so that I might repeat the wonderful experiments, and any who are interested may observe for themselves the delicacy of the device.

And from such simple beginnings has sprung the present system of telephonic communication with all its immense ramifications.

My purpose is not to relate the numerous and valuable achievements of Professor Hughes, but I think I must quote the concluding paragraph of his original communication to the Royal Society; it runs thus :—

"I do not intend to take out a patent, as the facts I have mentioned belong more to the domain of discovery than invention. . . . I have already my reward in being allowed to submit my researches to the Royal Society."

In the year 1893, Lord Rayleigh, one of the foremost physicists in Europe, was troubled in his mind about the density of nitrogen—he could not be quite sure of the value of one litre of this gas to the ten-thousandth part of a gramme! He had carried out some very elaborate experiments, had separated nitrogen from the atmosphere by the orthodox method of passing air freed from carbon-dioxide over red-hot copper, and, with all possible adjustments of temperature and pressure and great refinements in weighing, had obtained the figures 1·2517 grammes for a litre, but he also found that—if instead of taking nitrogen from air he obtained it by the decomposition of a chemical compound, ammonia for instance—the resulting gas was always about one-half per cent. lighter than it should have been, and he concluded an important communication to the Royal Society with these words :—

"Until the questions arising out of these observations are thoroughly cleared up the above number for nitrogen must be taken with certain reserve."

The observation attracted the attention of Professor, now Sir, William Ramsay, who was then working at University College, and a few preliminary experiments were made that gave surprising results. Taking atmospheric nitrogen and passing it through tubes containing red-hot magnesium, the bulk of the gas was made to combine with the metal, and the residue, although presumably nitrogen, was found to be decidedly denser than the original. Professor Ramsay immediately communicated with Lord Rayleigh, and the two investigators joined forces. Having obtained the clue, the work proceeded rapidly and soon indicated that the probable cause of the discrepancy in the weights of nitrogen was due to the presence in the atmosphere of a hitherto unsuspected constituent of greater density.

Such a result, startling though it was, had really been anticipated a century before by Cavendish, who had caused nitrogen to combine with oxygen, and had obtained a very small residue which the instrumental means of that day did not enable him to distinguish from the original nitrogen. Since then, however, the spectroscope has been placed in the hands of the chemist, and armed with that instrument of superb delicacy Sir William Ramsay was able to keep the gas under examination during the whole period of manipulation and watch the very inmost working of its atoms.

An ingenious system of tubes was arranged by which it was possible to cause a volume of nitrogen to circulate through purifying agents and over red-hot magnesium, and the gas was continually examined with a spectroscope. At first the usual pink glow of nitrogen was seen, but after a very large amount of gas had been absorbed the colours changed slightly and ultimately gave rise to a novel and beautiful spectrum, and after a deal of confirmatory labour the authors were able to announce the discovery of a new constituent in the atmosphere which they named "Argon."

But the story does not end here, the whole scientific world was talking about the new discovery and many suggestions were made;

among others it was pointed out that it had long been known that some rare minerals contained nitrogen occluded in some unknown way, and it decided to look into the question and see if this nitrogen differed in any way from atmospheric nitrogen. The experiment was soon made, the mineral clevite was taken, its gas extracted and subjected to spectroscopic examination, and lo! instead of the expected red glow of nitrogen the gas in the tube glowed with a golden yellow light, and the spectroscope revealed the presence of a line close to the Sodium line, a line that had been observed forty years before by Jannsen in the sun's photosphere, and had been considered by Lockyer and Frankland to be due to an elementary gas which they provisionally named helium! The gas contained in clevite was not nitrogen after all, but helium found for the first time in a terrestrial substance.

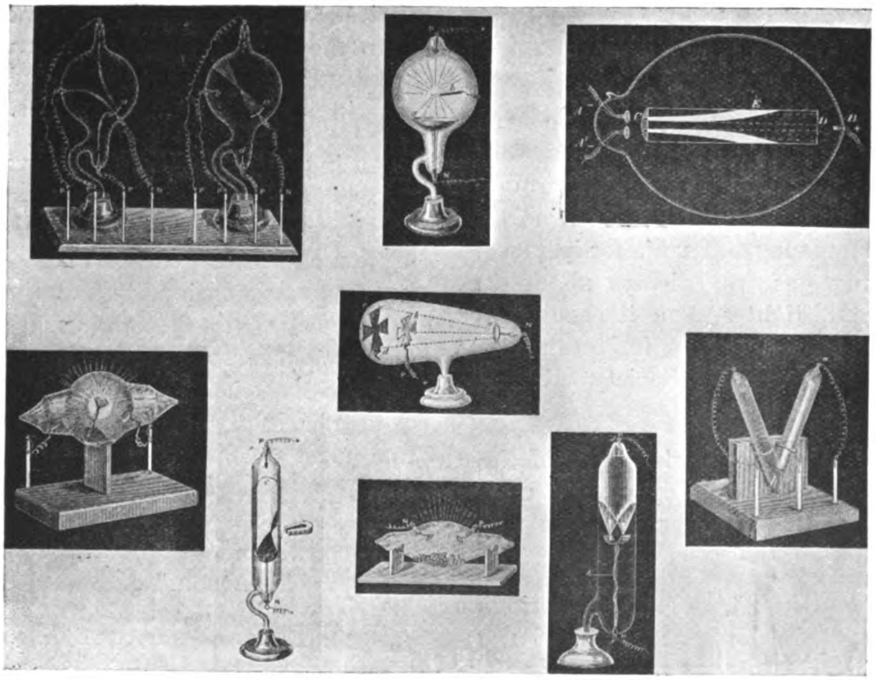
Nor is the story yet complete; further research upon the subject revealed the existence in addition to Argon and Helium of three other gases in the atmosphere to which the names Neon, Krypton and Xenon were given—forming that strange group of bodies now known as the "inert gases of the atmosphere."

And the whole of this wonderful series of discoveries originated in the fact that in 1893 Lord Rayleigh was troubled in his mind about the exact weight of a litre of nitrogen.

Through the kindness of Sir William Ramsay I am able to show you specimens of each of these gases of his own preparation and there may be an opportunity later to examine them with a spectroscope.

And now for my third illustration of the beginning of great things.

In 1879 that well-known English scientist under whom I have had the honour of working for many years—of course I refer to Sir William Crookes—gave a public account of his researches upon the passage



Radiant Matter Tubes devised by Sir William Crookes

of electricity through gases under the name of Radiant Matter. Time will not allow me to attempt to indicate what these researches really involved but they caused a very widespread interest, not only in this country but the world over. Those beautiful tubes showing the glowing of rubies, diamonds and other precious stones, under the impact of cathode-rays, the shadows thrown upon phosphorescent substances by obstacles placed in the path of the rays, the bending of the rays by the action of a magnetic field, the mechanical force produced at the point of impact, and the heat developed if the rays were brought to a focus by curving the cathode, these and many other beautiful phenomena arrested the attention of even the unscientific public. The great beauty of these Crookes' tubes was at once recognised by that business-like nation, who, at the present moment, are so vehement in their hatred for us, and soon they flooded the world with reproductions of Crookes' original tubes, so that ultimately some of them were to be found in pretty well every scientific establishment in the world and their wonders could be observed by one and all.

It was in 1895 that Professor Röntgen in Vienna, working with one of these tubes, made the happy discovery that it emitted these new rays that now bear his name. The tube he used was, as is well known, the one that was designed by Crookes to demonstrate the shadow thrown by an opaque substance on the end of the tube—and as we now know was not by any means the most suited for the purpose, but it chanced to be the one that Röntgen was using at the time.

As I have already pointed out Professor Herbert Jackson made an immense advance by showing that the curved cathode tube was the most efficient for the production of the new rays: thus we see that England has the honour of both the origin and the development of the X-ray tube.

And now comes a piece of history that is an almost exact parallel to the discovery of the inert gases of the atmosphere. Just as the observation of one engaged in pure physics attracted the attention of a chemist and led to the discovery of five new elementary gases, so here a chance word of a mathematician was the seed from which has sprung an entirely new science, the science of RADIO-ACTIVITY.

In the early days of the discovery of X-rays it was thought by many that the intense yellow phosphorescence at the end of the tube, where the cathode-rays struck, was the origin of the new radiation, and M. Poincaré, the French mathematician, made the suggestion that if the phosphorescing glass of the vacuum tube was the cause of the production of X-rays, might it not be that other phosphorescing substances also produced the rays. The observation arrested the attention of M. Henri Becquerel, the well-known authority upon this particular branch of science, who lost no time in putting the matter to a test. He took some of the double sulphate of uranium and potassium, a salt that is most luminous under the action of ultra-violet light, placed it on a photographic plate that had been previously covered by black paper and shut it up in the dark for a short period: upon developing the plate it was seen that rays had been given off that had penetrated the paper wrapping and impressed themselves upon the plate.

Apparently Poincaré's surmise was verified, but it quickly became evident that there was far more in the discovery, and soon M. Becquerel was able to announce the spontaneous radio-activity of Uranium and its salts.

That discovery, as is well known, immediately led to the research by the Polish lady Madame Curie which gave us Radium and its allied elements, and in the hands of our countrymen Professors Rutherford, Soddy

and others has brought into acceptance ideas that can only be mildly described as revolutionary, ideas that but a few years ago would not have been entertained, which have shaken the very foundations of chemical science, and has brought the Alchemists' dream of the decomposition and transmutation of the elements to our very doors.

If you have been able to follow me in these three sketches you will have seen that each of the three great developments in economics, chemistry, and electro-physics, which now form part of the life system of the world, had their origin in nothing greater than an individual determination on the part of three of our countrymen to advance a step further across the borderland of the unknown.

And the moral of all this is, that success does not come to the "lookers on," but, in the instances that I have related and many that I have not related, it has come as a reward of sincere and often very hard labour done for the work's sake.

And now about to-day and to-morrow, for to-day we may do well by gathering up a few of the advances of the last 20 years.

The little Crookes' tube, a laboratory-made scientific curiosity that needed to be handled with the greatest care, has grown into that magnificent piece of apparatus the modern X-ray tube, a tool that with moderate care is practically indestructible and through which can be passed with perfect safety one hundred times the current that would utterly ruin the small one.

The induction coil, except in a few instances that could be counted upon the fingers, a fragile machine rarely seen and still more rarely used, has given place to the modern high tension transformer with its flaming torrent of sparks.

The early radiograph of Mr. A. A. Campbell Swinton of a hand taken with wonderful success but with great difficulty and with an exposure of four minutes, has given place to the magnificent radiograph of a thorax by Dr. Robert Knox taken with a single flash and a probable duration of $\frac{1}{100}$ th of a second.

The X-ray, no longer an unknown form of energy connected with a series of more or less hazardous speculations, has fallen into place at the end of the scale of electromagnetic vibrations, and the lengths of the waves have been measured and tabulated.

The truth of the elaborate theories of crystal structure and space lattice has been rendered visible upon the phosphorescent screen and on the photographic plate.

And transmutation, the dream of the alchemist of old, has been found to be proceeding in our presence and can be observed by any one who possesses a speck of that extraordinary substance that we have called radium. An element with definite chemical properties, a well determined atomic weight and capable of giving a characteristic spectrum, is spontaneously changing into another totally different element, having different properties, a different atomic weight and capable of producing a spectrum of a completely different character. Am I dreaming? No, I do not think so. That pretty little instrument devised by Sir William Crookes which I have in my hand, contains a speck of radium which is continuously throwing off from its heavy atom, with a velocity approaching that of light, those α particles which collectively constitute helium. Each particle as it reaches the screen of zinc sulphide crystals strikes out a flash of light and can be observed by any one, and not only can this transformation be made visible, but the genius of C. T. R. Wilson has made it possible to produce a photograph of the track of an α particle from the moment that it leaves its parent radium with that terrible velocity, until, its energy absorbed and its electric charge neutralised, it comes to rest a harmless law-abiding atom of helium. The photograph that I am showing you is one of the most remarkable achievements upon record.

I have recounted a few of the most brilliant discoveries of the present day, but the words written by that old-world poet still remain true—

"And later times things more unknowne shall show."

We have a magnificent field of work at our very doors, and I suppose that we are all anxious to do our little, but the question often is, how are we to begin? I think this has been answered by our friend Rudyard Kipling in better words than I can command:—

"The wisest thing I suppose that a man can do for his land,
Is the work that lies under his nose with the tools that lie under his hand,"

sang the children in the school led by the philosopher man.

It is acknowledged that the medical man must be fully acquainted with the physics of X-rays to get the full value from their use, and when we turn to the field of Radium-therapy, both physical and chemical knowledge becomes not only *essential but absolutely necessary*. The brilliant exposition of the chemistry of radio-active bodies and their transformation products given to the Society by Mr. Alexander Fleck, and the papers that have been contributed by Sir Ernest Rutherford, Professor Soddy and others, cannot fail to be of great use.

The therapeutical value of the radio-active bodies and their salts and solutions is unquestionable, but the greatest caution is necessary in the interpretation of the results. The Society will do well in discouraging by every means in its power the exploiting of radio solutions or treatment by interested persons unless carrying unquestionable authority.

At the moment we have in our hands the question of protection of X-ray operators from harmful effects that may follow from want of care upon their part during work, a question of very great seriousness at a time when all radiologists are working at high pressure.

This matter, important as it is, is only upon the surface of a much greater one, that of the exact understanding of the action of radiations upon living tissues. The recent valuable contribution by M. Clunet dealing with the histological changes produced by X-rays was a step in the right direction; but for the war we should probably have had other investigations of a similar nature, but this should only make those who are forced to stay at home increase their effort.

The recent advance in our knowledge of the actual nature of the radiations that are produced by the sudden stoppage of the cathode stream have made very refined determinations possible. Röntgen rays are no longer an unknown or even uncommon form of energy; it is practically certain that they differ from the familiar electro-magnetic disturbance of light, heat and electricity only in the extreme shortness of their waves, which range from about 0·5 to 2·0 ten-millionths of a millimeter. It is now possible to produce and maintain rays of any desired value, and if we consider the immense differences in the effects that are produced by luminous radiations differing only slightly in their wave lengths, it does not seem unreasonable to expect that similar special re-actions may be due to shorter rays of some definite quality.

But the matter calls for investigations of very great refinement and the man who is going to make the desired advance will have to combine the qualifications of a pathologist with a thorough knowledge of the physics involved in the passage of electricity through gases. I have heard it suggested that it may not be impossible in the near future to provide the medical man with a set of tubes duly labelled for the production of any desired kind of rays; this looks very enticing although I myself doubt if it will be possible or even desirable. I think there are too many conditions involved to enable X-rays to be put up in bottles like drugs, and for the purpose of exact research such a plan is unthinkable.

In the purely physical sphere of our activities the field is equally enticing, the "Crookes' tube" has not yet yielded up all its secrets. Approaching the limit of luminous radiations, the vision for most people ends in the blue violet with waves of about 0·4 thousandths of a m/m or 4,000 Angstrom units. Shorter waves than this take us into the ultra-violet and Schumann region where chemical and ionising actions reach a maximum. Schumann extended his photographic record to close upon 1,000 A.U. and recently Layman in America has obtained records of even shorter waves in an atmosphere of helium having reached 600 A.U. Beyond this and before we reach the softest X-radiations lies a hiatus, a dark continent of the deepest interest to the physicist, for in that region take place the profound changes that distinguish luminous radiations from those produced in a vacuum tube, and happy will be the man who first lights it up. There may be found the secret of many things that at the present time cannot be explained.

At the other side of the vacuum-tube rays, between those that we call "hard X-rays," and before we reach the γ rays of radio-active substances lies another though smaller gap. Here much work is being done at the present time, and there is reason to believe that before long it will be possible to construct tubes that will give rays closely approaching if not corresponding to γ rays in wave-length.

The methodical researches of Barkla and Sadler, the brilliant work of Bragg, the interesting observations of Chambers and Rankin and many others, indicate very plainly that fruitful discoveries are waiting for those who will but reach out the hand to take them ; but the unknown borderland must be approached soberly and above all with an honest admission of one's own ignorance. Thus only can we prove the truth of that old saying that "Nature never did betray the heart that truly loved her." Many years ago Sir Henry Roscoe, in concluding his presidential address to the British Association for the Advancement of Science, used some words that I have never forgotten, and which at the present time, when it is the obligation of each one of us to do our best, I feel that I cannot do better than repeat for the encouragement of us all :—

"The worth of a man lies not in the truth which he possesses or believes that he possesses, but in the honest endeavour which he puts forth to secure that truth."

LIST OF EXHIBITS AT THE OPENING MEETING.

EXHIBITED BY

MR. CUTHBERT ANDREWS,

47, RED LION STREET, HIGH HOLBORN, W.C.

A Mammoth or Moment type X-ray Tube for heavy discharge work.

A Water-Cooled X-ray Tube for use in any position above or below the couch, suitable for all classes of work.

A Heavy Anode X-ray Tube made of British glass, having all the characteristics of the best German glass. These tubes showed the characteristic green fluorescence, and were manufactured throughout in London.

Various protective articles for use by X-ray workers, comprising Face Mask, Apron, Gloves, &c. Also a sample of a new grade of protective material, having an opacity equal to about 15 per cent. more than the similar materials hitherto used.

The Walter type of Radiometer for estimating the penetration value of X-ray Tubes while working.

EXHIBITED BY THE CANCER HOSPITAL,

FULHAM ROAD, S.W.

A series of radiographs of the Thorax and of the Intestinal Tract after barium meal.

EXHIBITED BY THE MEDICAL SUPPLY ASSOCIATION.

"The Kompact" No. 2 X-ray Installation for universal X-ray work, a complete installation, including all that is required and one gross of photographic plates, price £130.

Blake's Localiser.

The Vibreur of Professor Bergonie for locating particles of iron and steel in the human body by means of an alternating current electro magnet.

Benoist Crypto Radiometer.

Slide Rule for calculating exposures.

EXHIBITED BY HARRY COX & CO., LD.

X-Ray Couch or Table.

X-ray couch, with tube box, capable of taking the largest tubes, protected with 2 mm. of sheet lead, suspended from a ball-bearing trolley, which has longitudinal and transverse motions, controlled by a lever at the left hand of the operator. On this lever is also carried the control for a rectangular diaphragm, with thoroughly opaque leaves opening up to 5 inches square. An apron board is fitted at the side of the couch and moves with the tube box. This is covered with a further 2 mm. of lead, forming absolute protection for the operator. A mirror in the apron board and a lead-glass window in the tube box enable the operator to examine the tube at any moment without stooping down or exposing himself to the rays. X-ray tubes are mounted and accurately centred on tube slides before being placed in the tube box, in which they make automatic connection. High tension leads are arranged on one end of the couch only, thus keeping two sides and one end free for operator and patients. The top is of 3-ply wood, but is removable, and can be replaced by canvas if and when desired. An anode finding device is mounted, so that a plumb bob shows the position of the anode at any time. A plate-holding device for conveniently holding plate or cassette above the patient is provided. The couch is made in polished oak and highly-plated steel tubing.

New Protective Tube Stand.

This tube stand carries a protective tube box which is accurately counterpoised whatever its position. There is only one adjusting screw, which clamps it in position when the operator has placed it exactly where he wants it. There are no laborious racking motions, and any angle and position can be obtained with the utmost ease. It is constructed almost completely of wood and other non-conducting substances; the nearest metal is 18 inches from the tube box.

Fluorescent and Intensifying Screens.

The latest forms of these screens were shown. Their surface is absolutely perfect, and the results obtained are entirely free from grain. The rapidity of the intensifying screens is much greater than that of any other make.

EXHIBITED BY MESSRS. NEWTON & WRIGHT, LD.

The hydrogen X-ray tube recently placed on the market by the Snook-Roentgen Company in Philadelphia. The following short description of the tube may be of interest.

The tube is constructed with a very heavy copper anti-cathode with solid tungsten plate and it is exhausted by a special process which leaves within the tube a very small trace of pure hydrogen gas only. This process claims to produce a tube which is a more efficient source of X-rays than an ordinary tube; the vacuum remains constant even when heavy currents are used.

Comparing the equivalent spark gap of the hydrogen tube with an ordinary one the penetration of the rays given out appear to be one degree on the Benoist scale more

penetrating. The tube is fitted with two auxiliary bulbs containing palladium osmotic tubes connected up by a terminal. When it is required to lower the vacuum the negative pole of the coil is connected to one of these terminals, and if it is desired to raise the vacuum the negative wire is connected with the other auxiliary terminal. A double spark gap device will be ready shortly which will facilitate these connections and enable the vacuum of the tube to be controlled either up or down without switching off the high tension current.

DR. THURSTAN HOLLAND'S LOCALISING SCREEN.

DR. HAMPSON'S LOCALISING SCALE.

These have been fully described in this JOURNAL, Vol. XI., 42.

A NEW PATTERN OF DR. HAMPSON'S RADIOMETER FOR ACCURATELY MEASURING THE PASTILLE DOSE.

The improvement consists in it being arranged to accommodate the pastille mounted on small slips of cardboard which in turn serve to support the pastille when in use in the treatment stand. This new pattern renders unnecessary the small tweezers which were hitherto supplied to manipulate the pastille.

PROF. SALOMONSON'S PENETROMETER.

This instrument is perhaps the most accurate and convenient to use of all the radiometers depending upon the comparison of varying thicknesses of aluminium against the standard silver plates.

MESSRS. KODAK, LIMITED, KINGSWAY, LONDON, W.C., distributed a number of negatives, showing the properties of their recently introduced X-ray Films. *See* Vol. XI., No. 45, p. 125.

EXHIBITED BY
MESSRS. WATSON & SONS
(ELECTRO-MEDICAL), LIMITED.

NEW MODEL COUCH DESIGNED BY SIR JAMES MACKENZIE DAVIDSON.

The couch is constructed in mahogany with a three-ply wood top without any obstructions at the edges, so that plates and exposure cases can be freely slipped under the patient, if it is desired to take a radiograph with the tube above the patient.

The tube box is octagonal in shape, lined with protective rubber and fitted with a self-centring tube grip, which will easily take tubes up to 8 inches in diameter. The box is mounted upon a platform which carries a small upright to which is attached an arm fitted with a special cross wire attachment, which can be used in conjunction with the new model Mackenzie Davidson Cross Thread Localiser. This attachment provides the usual cross markings, and also arranges for the marking of the skin of the patient, whilst it permits any size of plate to be used. Both the longitudinal and transverse movements of the platform carrying the tube box are on ball-bearings and move with great freedom. The transverse movement is secured by a winch and cord so that the tube may be shifted into any required position without stooping. The longitudinal movement can at the same time be regulated with the same hand, the whole arrangement permitting of a very rapid screening examination of the whole body.

By means of a director and the cross wires provided, a tube can quickly be adjusted in the box, so that the vertical ray from the anti-cathode can be obtained in the centre of the diaphragm. This obviates the necessity of a plumb bob, and absolute freedom of movement of the patient is provided for by the hinged joint, which enables the horizontal arm to be instantly folded out of the way. Scales are provided upon the upright and upon the transverse movement of the tube box, as well as on two slotted

supports, which enable the height of the tube-box to be adjusted, so that the distance between the anti-cathode and the screen could be varied at will. An automatic stop is provided which gives the correct movement of the tube in the longitudinal for localisation or stereoscopic work.

The tube box is lined with thick protective rubber, and in addition, an adjustable shield is fitted on the small upright which carries the winch and cord, and this travels with the tube, so that a double protection to the operator is thus secured.

LOCALISATION CALLIPERS.

This instrument is designed to save time in interpreting double shadow localising radiographs.

The points are opened by the side screw until they are in register with corresponding portions of the shadows of the object whose position is required, and the depth can then be read off at once in inches upon the dial.

It is constructed so as to be entirely without "back lash" and the zero does not require resetting when once adjusted. It can be supplied for any tube distance or shift.

The instrument has been designed by Captain C. E. S. Phillips.

THE SHUTTER SCREEN.

The Shutter Screen is useful where a vertical screening stand is not available. It is specially applicable where only a very limited space is available such as on shipboard. The slit when held with its greatest length vertically enables the Aorta to be efficiently examined and when held horizontally, the apices of the lungs, movements of the heart and diaphragm, etc.

The Screen can be conveniently slung from the ceiling where necessary.

It was made to the design of Captain C. E. S. Phillips.

ORDINARY MEETING.

AN ORDINARY MEETING was held at the Institution of Electrical Engineers, on December 7th, Mr. J. H. Gardiner, F.C.S., President, in the Chair.

The minutes of the last meeting were read and confirmed.

The following were elected members of the Society:—

Mahomed Kamel Aly, Alexandria.

F. Nesfield Cookson, M.D., Stafford.

A. St. George Caulfield, Limington.

Walter John James, Haverstock Hill, N.W.

Leonard A. Levy, M.A. (Cantab), F.I.C., F.C.S., Cricklewood.

James B. Waters, M.D., Sunderland.

Miss E. M. White, M.R.C.S., L.R.C.P., Endell Street, W.C.

Frank Bennett Young, Cotham, Bristol.

Dr. Cirraco Yrigóyen, San Sebastian, Spain.

The following papers were then read and illustrated by their authors:—

SOME REMARKS ON FLUORESCENT AND INTENSIFYING SCREENS.

By LEONARD A. LEVY, M.A. (Cantab.), D.Sc. (Lond.), F.I.C., F.C.S.

Fluorescent and Intensifying Screens—familiar as they are to all radiographers—are perhaps apt to be regarded simply as necessary adjuncts, and are not considered as essentially possessing any marked peculiarities. This from the point of view of the chemist, is quite an incorrect standpoint. The subject of fluorescence and phosphorescence is still so obscure and so empirical, that a few remarks on these properties as they affect the Rontgenologist may perhaps be of interest. The subject of fluorescent screens may be considered first.

These are of course familiar to everybody who has ever worked with X-ray apparatus. A considerable number of different chemical substances have been employed from time to time in order to produce the active or fluorescent coating. The substance which has by far the most extensive application, is the familiar barium platinocyanide. Now it might be considered that in order to make a platinocyanide screen, it was only necessary to obtain a supply of the fluorescent substance and in some way or the other to spread a uniform coating on a piece of cardboard or other base, to which the salt is firmly attached by some adhesive. In actual practice however the matter is by no means so simple as this. To begin with barium platinocyanide is a very remarkable substance, possessing many peculiar, and from the point of view of the screen maker, disagreeable properties. The salt used for fluorescent screens is a double cyanide of barium and platinum containing four molecules of water of crystallisation.

$$Ba\ Pt\ (CN)_4\ 4\ H_2O$$

This substance is capable of existing in three forms, all of which are chemically identical, but which nevertheless display totally different physical properties.

About twelve years ago the author was endeavouring to prepare some barium platinocyanide for certain experiments on fluorescence, but he found that the product obtained was not at all efficient in this respect. The fluorescence which it displayed under X-rays or ultra-violet light was very poor indeed.

The reason for this peculiarity was sought, and eventually the author was able to prepare two totally distinct crystalline varieties of barium platinocyanide, both of which gave identical results on quantitative and qualitative analysis. It is not necessary to specify all the details which led to this conclusion; Fig. 1 shows in tabular form the main differences between the two salts, which are of interest.

VARIETIES OF THE TETRA-HYDRATE OF BARIUM PLATINOCYANIDE.

$$Ba\ Pt\ (CN)_4 \cdot 4H_2O$$

PHYSICAL PROPERTIES.	CRYSTALLINE A.	CRYSTALLINE B.	AMORPHOUS.
COLOUR.	Orange.	Apple Green	Brick Red.
CRYSTALLINE FORM	Identical	Identical.	None.
FLUORESCENCE	Very feeble	Very brilliant	None.

Fig. 1.

TABLE SHOWING THE PROPERTIES OF THE TWO FORMS OF BARIUM PLATINOCYANIDE.

It will be seen from the Table (Fig. 1), that the two crystalline varieties of barium platinocyanide differ markedly in their colour and in the intensity of the fluorescence which they display and this effect is shown very clearly if two screens are prepared, both containing the same weight of salt per unit of area, from the two crystalline compounds. Careful measurements show the ratio of the intensity of the fluorescence displayed by the yellow and green varieties to be in the proportion of 1 to 50, the green salt being 50 times as efficient in this respect as the orange-yellow crystals.

These two varieties of barium platinocyanide are worthy of further consideration. They have the same chemical composition, but differ markedly in certain of their physical properties, such as colour and fluorescence. They also differ in their specific gravity. The question therefore arises, to what cause are these differences to be ascribed? As a result of a large number of experiments, the author has come to the conclusion that the differences must be due to a special case of stereoisomerism, that is to say to a difference in the method in which the individual atoms, molecular groups and molecules of water of crystallisation are arranged around the central platinum atom.

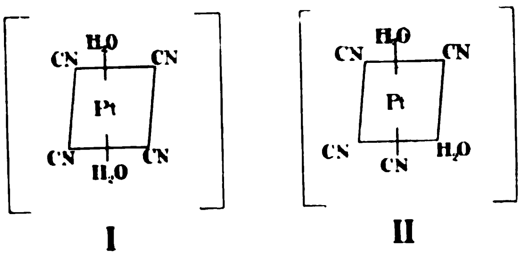


Fig. 2.

STEREOISOMERISM OF BARIUM PLATINO-CYANIDE.

Modern theory postulates the attachment of the water of crystallisation in many compounds partly on the basic and partly on the acidic radicles (c.p. sulphates with water). If the assumption be made in the case of the isomeric tetrahydrates of barium platinocyanide, that two molecules are associated with the barium and two with the platinocyanide grouping, we can represent the existence of the two modifications by the formulæ shown in Fig. 2.

Here the platinum is supposed to be situated on the centre of a regular octahedron, the cyanogen and water groups being situated in the two different positions. It is conceivable that the unsymmetrical form II. may be incapable of that movement under the stimulation of the incident radiations, while in the case of the other variety, represented by the first formula, they are able to set up the vibration to which the fluorescence phenomena are due.

We come now to the consideration of the third form of this substance.

This is a form of the salt, which is also familiar to X-ray workers. It results from the action of X-rays or from mechanical pressure on either of the crystalline modifications, and is exemplified by the change of colour which occurs when Sabouraud pastilles are used for the measurement of dosage. It must not be confused with the reddening which occurs if a screen is accidentally left in a hot place. This is due to dehydration.

If some of the crystals of the green or yellow salt are powdered in a mortar, the powder turns brick-red and at the same time loses its fluorescence.

This brick-red material is the amorphous or non-crystalline form of the tetrahydrate of barium platinocyanide, and it displays no fluorescence whatsoever.

If a piece of cardboard be coated with a layer of the green salt without the additional protection of a coating of varnish, it will be seen that, on striking or rubbing the surface of the salt, the fluorescence is totally destroyed, and the struck or abraded portions appear as black marks upon the luminous surface, when the screen in exposed to X-rays. It is well known that the same action takes place under the action of X-rays, but it would require an enormously long exposure in order to effect the same total destruction of the fluorescence which can be effected by a mechanical blow or abrasion.

It is usually said that the change from the green fluorescent to the red and non-fluorescent salt is due to dehydration. This is apparently borne out by the fact that the dehydrate of barium platinocyanide, with two molecules of water of crystallisation instead of the usual four, is brick-red and non-fluorescent. This, however, is not the correct explanation—the real cause is the change from the crystalline to the amorphous condition, and the effect is the same whether produced by X-rays or mechanical means. It is, indeed, very difficult to see how the effect of a mechanical blow could possibly be the cause of the reduction of the state of hydration of the salt.

The author has found that this change of state from crystalline to amorphous, which also means the progressive destruction of the screen, can be inhibited by the use of a special method of preparation of the salt. For this reason the salt used for screen making is totally different from the salt employed for exposure pastilles. In the

latter case a substance is required to be specially sensitive to the action of the rays, whereas the possession of this property is a considerable defect in a screen.

If two pieces of cardboard are coated with barium platinocyanide, one with the sensitive form employed for pastilles and the other with a non-sensitive form which the author employs for screens, it will be found that on exposure to X-rays, the screen is scarcely affected whilst the pastille preparation turns to the full B tint.

The fluorescence displayed by the two preparations is originally the same, but after exposure the fluorescence of the pastille preparation is poor in comparison with that still displayed by the screen.

It is a very curious fact that X-rays, which are now acknowledged to be composed of regular ethereal waves of most minute wave-length, should have the effect of converting the crystalline form of barium platinocyanide into the amorphous, whereas ethereal radiations of a much greater length, that is to say, blue and ultra-violet light waves, have the opposite effect.

Zinc sulphide is another body which has been used to a small extent for screen making. Ordinary zinc sulphide is obtained by precipitation of a solution of zinc salt with a soluble sulphide, and in this form displays no interesting luminous properties.

Zinc sulphide can however be prepared in the form of hexagonal crystals and is then known as hexagonal zinc blende. In this form it displays fluorescence and phosphorescence and other luminous characteristics, and finds its chief application in the admixture with radium salts for the manufacture of luminous dials for watches and scientific instruments.

This form of the substance displays a considerable after-phosphorescence, which renders it quite useless for screen making. This phosphorescence can be destroyed by suitable treatment and a fluorescent substance results which displays no phosphorescence. Screens made of this material respond very well to very soft radiations and show the details of flowers, very fine pieces of aluminium, etc. They are however practically useless with hard radiation. For this reason and also because of the peculiar orange colour of the fluorescent light, these screens have found but little application. A very peculiar property of this substance is the triboluminescence, or luminosity when crushed or cleft, which it displays. A small quantity of the powdered material, if gently rubbed in a glass mortar with a pestle, emits luminous flashes of yellow light which display a momentary but very considerable luminosity.

Owing to the great improvements effected in their manufacture in recent years, intensifying or accelerating screens have a great and continually increasing vogue.

The intensifying screen operates by converting the energy of the X-ray radiation into that of luminous radiations, which are far more potent in effecting the necessary chemical change on the photographic plate. Although only a portion of the X-ray radiation is converted—the remainder passing through the screen—yet owing to the vastly more powerful effect of the luminous radiations, exposures can be reduced to at least one-tenth of the normal amount required without a screen.

The chief requisites of an intensifying screen are :—

(1) It should effect a considerable reduction of the time of exposure.

(2) It should yield negatives free from grain and blurring.

(3) It should not display any appreciable after phosphorescence, as this invariably leads to fogging of the negative.

The author does not propose to say much about the fluorescent materials employed for the manufacture of intensifying screens. The salt employed, calcium tungstate, is specially chosen to yield fluorescent light of a blue colour, as this has by far the most effect chemically upon the sensitive emulsion. The radiographs which accompany this communication have been made in order to determine the best method of using intensifying screens.

The author is well aware that these details are well known to all experienced radiographers, but he has a definite object in giving as much publicity as possible to them. A very large number of X-ray outfits are being used in the various military and Red Cross hospitals all over the country, and a certain proportion of the operators are not very experienced, and some have no knowledge at all of intensifying screens. For this reason the author has had a series of radiographs made under the varying conditions of exposure, hardness of tube, time of development and so forth. The conclusions arrived at are those already known to you, and they are described in "Radiography and Radium Therapy," by Dr. Robert Knox. Certain of the results may, however, be of interest as they show how with one and the same intensifying screen, one and the same operator and the same subject, results can be obtained of first class quality in the one case and very poor quality in the other case. For effects like these the screen is often blamed whereas the fault attaches not to the screen and not necessarily to the operator. For purposes of comparison the same subject has been taken in every case. The differences are more noticeable in the original negatives, and they are well shown in the reproduction, though some of the detail is of course lost. In every case the same "London" Intensifying Screen was used. Plate II., Fig. 1, is a radiograph of a shoulder taken without a screen, the normal exposure being given. A 6-inch Tungsten target tube was used worked at about 6 Benoist.

Plate II., Fig. 2, is the same subject taken with the same tube and under the same conditions except that a "London" Intensifying Screen was employed. The exposure was reduced to one-tenth of the normal and was made through the screen. The contrast observable on the original negative is superior to that in the negative obtained without a screen.

Plate II. Fig. 3.

This is again the same subject taken with the same tube and screen. In this case the exposure was made through the glass plate, the necessary exposure being one-sixth of the normal. There is very little difference in the original negatives between the one taken through the screen and the one taken through the glass of the plate. The contrast in the one taken through the glass is not quite so good and there is a great difference in the time of exposure. For this reason exposures should always be made through the screen.

Plate II. Fig. 4.

This is a very bad result indeed and the author wishes particularly to draw attention to it. This was taken by the same operator with the same screen, and he was actually endeavouring to obtain a good result.

The conditions of the exposure were that it was made through the screen, but the latter was not dusted and both the negative and the screen were rather badly fingered. A hard tube was used and the negative over exposed.

Plate III. Fig. 5.

These radiographs show the effect of varying exposures respectively equal to $\frac{1}{20}$, $\frac{1}{10}$ and $\frac{1}{5}$ of the normal exposure. They were made with a 6-inch Tungsten target tube working at 5 Benoist.

The $\frac{1}{20}$ normal exposure would be improved by intensification of the negative. The $\frac{1}{5}$ has a tendency to graininess due to over exposure and the $\frac{1}{10}$ is about correct.

The next three sets of radiographs were taken to show the effect of varying the hardness of the tube.

Plate III. Fig. 6.

These radiographs were taken with a Coolidge tube of 3-inch hardness. The picture on the left was taken with a screen and the one on the right was made without a screen. It will be noted that the ribs appear much more clearly in the negative taken with the screen.

Plate III. Fig. 7.

This is again the same subject but taken with a Coolidge tube of 5-inch hardness. As before the picture on the left was taken with a screen and the one on the right was made without a screen. The best detail in the ribs is seen in the negative taken without the screen, but the details of the shoulder are best seen in the negative taken with the screen.

Plate III. Fig. 8.

The same subject taken with a tube of 7-inch hardness. The original negative taken with the screen shows a considerable amount of grain, and is generally foggy. The negative taken without a screen shows much more detail in the shoulder.

From these results the following conclusion can be drawn as to the best methods of employing intensifying screens :—

(1) The radiographs should always be taken through the screen.

(2) Great care should be taken to avoid dust and finger marks. A camel hair brush should always be used to dust over the surface of the screen before use.

(3) Over exposure should always be avoided. The latitude of exposure is considerably less with an intensifying screen. The correct result is obtained with one-tenth of the normal exposure, but quite good results can be obtained with considerably less than this, especially if the negative is intensified after development.

(4) The tube used for any given subject should be slightly softer than it would be for a normal exposure without a screen. For instance, where a tube of 4-inch hardness would be used without a screen the same tube should be worked at 3-inch hardness when used with a screen.

(5) Hard tubes give poor results and should *never* be employed with a screen.

The author is much indebted to Mr. Harold Stenning for his assistance in making all these tests with intensifying screens.

DISCUSSION.

Mr. F. H. Glew said: I think that the subject of phosphorescence and fluorescence is one that has been very largely neglected. Dr. Levy has been exceedingly modest, because the various results which he has shown us are really his own work.

The actions of phosphorescence and fluorescence is obscure. In some cases it is reversible by light, usually of a higher order of refrangibility. Phosphorescence is quenched by red light. If we take a phosphorescent body and expose it to direct sunlight we really get less phosphorescence than if we expose it to the blue sky. Of course, what is happening is that the blue light of the sky is comparatively free from rays of long wave length, and in sunlight the quenching action is proceeding at the same time as the excitation goes on. In dealing with X-rays we are working indeed, as we now know, with rays of very short wave length. Is it that the crystals are being ionised? It is a fact that in all cases where one has phosphorescence, one has an enhanced effect if the substance is really crystalline. If you take a crystal of white sapphire and expose it to radium, you certainly ionise it; if you then put that in boiling water, the deep sherry colour disappears. In reversal you get luminosity—phosphorescence—produced while the change

is going on. Is that so in the calcium tungstate screen? Then again—a practical point—what is the exact thickness necessary to get the maximum effect? If too thick it should be used behind the plate.

DR. G. B. BATTEN said: With regard to fluorescent screens, some of these seem to change in colour by use—I mean that they permanently change in colour. I have a very big screen which I got many years ago, and which has got slightly brown. It has lost its green look. I have tried putting it in daylight, and in subdued light, but it does not seem to alter back to its pristine colour, nor does it fluoresce as it used to do. I should like to ask Dr. Levy if there is any means of restoring it. A well-known maker in London asked me to test some screens, and he had a new kind of screen which fluoresced very brightly for quite soft rays, and gave beautiful pictures of things like flowers, but was no good even for the hand, where it did not seem to show any detail at all. Coming to pastilles. I did a lot of epilation work in ringworm cases long before pastilles were introduced, and I was most grateful eventually for having the help of the pastilles. I quite agree with Dr. Finzi, however, that the work is difficult; if the pastilles could be made to change a little more quickly it would be very desirable. If a more sensitive standard could be set up we should all be very thankful. The question of the standard colour is very important because it is so extremely easy to overdose the patients in treatment, and if one set of pastilles is 25 per cent. more sensitive than another we are heading for disaster. Coming to the question of intensifying screens, I quite agree with Dr. Finzi that if one has an apparatus good enough, and can keep the patient still enough, one can get a much better photograph without a screen than with one. In the old days what we sought for was contrast. In those days we were glad to be able to see a stone in the kidney at all, and therefore we were glad to use an accelerating screen, but with modern apparatus, if we can put enough current through the tube we get better detail without a screen.

DR. N. S. FINZI said: You will all agree with me that Dr. Levy has taught us a great deal this evening. The paper has been a very interesting one, and I for one wish it had been longer. It interests medical men considerably, both as regards treatment and radiography. With regard to trea'ment, the hastening of the rate of change of the pastille would be a very great improvement. The pastilles change so slowly at the present that we do not consider it safe to measure it directly on the skin, the change in colour being so very slight, even if Dr. Hampson's radiometer is used. I should like to ask Dr. Levy whether he regards the change in colour as an arithmetically progressive one or if he agrees with Holzknecht that the change is proportional to the square of the amount of rays received by the pastille. As to the intensifying screen, this is an extremely useful article in its right place, but it is not useful for everything. I am going to criticise these radiograms which Dr. Levy has shown. I maintain that the original radiogram taken without the intensifying screen does not show sufficient detail; it is not good enough. If you get a negative showing finer detail than that, and then try to get the same result with the intensifying screen, you blur that detail. I once made an experiment. In the same square of radiation I photographed two hands, one with an intensifying screen, and the other direct on a glass plate. All the conditions were similar, except that the exposure on the one side—the intensifying screen side—was one-tenth of that given on the other. The amount of blurring in the hand taken with the intensifying screen is very considerable compared with the extremely fine detail that one got on the plate with the correct exposure. Of course, it may

have been due to the particular intensifying screen I was using, but I have compared several different screens, and I have not been able to prevent this blurring with any of them. For fine detail in the lungs—rapid chest work—the intensifying screen should not be used. If we can get a full exposure and a rapid exposure, as we can with modern apparatus, we get a better result without the intensifying screen, even though the exposure be not so brief. It is rather a question of contrast than detail. With an intensifying screen and a short exposure one can get contrast, but not great detail. With regard to prolonged fluorescence, I have examined a screen, and have found phosphorescence 24 hours after the exposure has been made. Better results are gained if Ilford ordinary plates are used with an intensifying screen than if the Ilford X-ray plates are used.

THE PRESIDENT said: The paper is one that appeals to me very particularly, but I do not want to burden Dr. Levy with many more questions. I share Dr. Finzi's regret that a little more detail has not been given on some of the points, particularly on the constitution of the molecule that brings about such remarkable changes. I hope also that Dr. Levy will explain the experiment with the two tubes, and will say whether it is merely a matter of raising the temperature that enables the first to turn in one way and the second in the other. The remarkable effect obtained on rubbing the barium platino-cyanide screen was very striking, and one cannot help feeling the seriousness of the question when barium platino-cyanide is used so largely in dosage. As to intensifying screens, I agree with those who have spoken so far as the matter of contrast is concerned. Intensifying screens will give wonderful photographs, but the exquisite detail possible in X-ray photographs under perfect conditions cannot possibly be retained by the use of the intensifying screen. In the case of X-rays the radiation goes right through the film, and we get the action on the grains throughout the whole of the film body. The effect of the intensifying screen is more like that of light—acting upon the surface.

REPLY TO DISCUSSION.

DR. LEVY, in reply, said: First of all Mr. Glew asked me if I had any theory of phosphorescence and fluorescence. When I first started on the question, what I was really after was to solve the problem of why platino-cyanides should fluoresce while ferrocyanides for example do not. I started out full of hope, expecting to settle the matter in the course of a few weeks, but I have finished up in the other direction. I do not know the reason, and I am quite sure nobody else does. Various theories have been put forward to account for the fluorescence displayed under ordinary ultra-violet light by many organic compounds. There is a theory, propounded some years ago, that this was due to the oscillation of an atom or a molecule of the compound about its mean position. The fluorescence resulted from the oscillation. There is, however, no way of proving this interesting hypothesis. The next question I was asked had reference to the crystalline form. Was it a fact that the fluorescent properties were generally better displayed when the substance was in the form of crystals? This is not so in every case. There are bodies displaying a very powerful fluorescence which have no crystalline form, and which are vitreous in character. Taken on the whole, however, those bodies which display phosphorescent and fluorescent properties in the best degree are always crystalline, and are usually in the form of very well defined crystals. With regard to the thickness of the coating on the intensifying screen, the result is purely empirical. I simply tried different coatings until I found the one that gave the best result. The next point was about the action of the after-phosphorescence of tungstate screens. In cases where these display after-phosphorescence, I have

found that the hand warmed slightly and held behind the screen resulted in a distinct augmentation of the after-phosphorescence where the screen was warmed by the hand. That is simply an observation; I have no explanation to offer. With regard to the points Dr. Finzi raised, perhaps the most important had reference to the sensitiveness of the pastilles. It is perfectly possible to make a pastille considerably more sensitive than the pastilles at present in use. The question is whether it is desirable. Everybody has got used to the present standards, and a change would not be justified without strong reasons. Nothing less than twice the rate of change would in my opinion be worth while. It would be better to work to the existing standards. I cannot answer the question regarding the darkening of a pastille increasing in arithmetical progression or according to the square root of the X-rays, as I do not know, and I have not done any work on that line myself. I am not competent to judge the radiographs which Dr. Finzi criticised. I can only say that they were made with quite a small outfit just a 12-inch coil—and that might account for the slight defects which Dr. Finzi pointed out. With regard to the experiments Dr. Finzi carried out, he said that the result on the screen was inferior owing to blurring. Could not that be due to the fact that, apart from the length of exposure, the two were taken under the same conditions? This would mean that there was the same degree of hardness of tube in both instances, and thus in one of the radiographs taken with a screen an inferior result would be obtained. The tube should be softened when an intensifying screen is being used. After-phosphorescence can persist any length of time, from a few seconds up to twenty-four hours or more. The screen I showed this evening displays a very faint after-phosphorescence for a few seconds, and that is desirable because to a certain degree it increases the rapidity of the exposure. What has been said as to the superior value of the ordinary plate over the X-ray plate under these conditions is quite correct. I believe that the screen plate is simply a plate which is made specially sensitive to the blue-violet light of the intensifying screen, and experiments made with screen plates give just about the same results as with X-ray plates, but I brought along negatives made with X-ray plates this evening, because I thought that, as most people are using X-ray plates, the results would be comparable. With regard to what was said by Dr. Batten about the colour of his screen, I may tell him that his colour change is permanent; he will not be able to restore the pristine colour. He has probably got so much platinum on his screen that he could get a new one without any financial loss. He mentioned a screen which was excellent for showing flowers and very thin objects, but worthless even for the radiography of the hand. It was a zinc sulphide screen made by myself and was discarded for that particular reason. It did fluoresce, but it was not of much use for anything save the finest objects. One could see the diminutive milligram fractions from a box of weights with that screen, and this cannot be done with any other. With regard to the best method of turning the pastilles back, the reason that direct sunlight seems to be so prejudicial is because the barium platinocyanide will not stand a considerable rise in temperature. The effect of a quite small rise in temperature is often confused with the effect of the light itself. The same thing happens if a fluorescent screen be left near a hot water pipe all night. I was asked as to whether the intensifying screens were all made of calcium tungstate. So far as I know they are all made of calcium tungstate, but some of this material as used for screens is scarcely worthy of the name. It may contain 100 per cent. of that material and be useless for screen manufacture. Lastly, with regard to the points raised by the President. The rubbing of the pastilles would not be serious at all, because

all the pastilles used have a protective coating of varnish on the front. You would not get discoloration due to rubbing. Then with regard to what has been called my Maskelyne-Devant experiment, there is nothing really mysterious about it. In fact, the only "trick" about it is the heating, which one has to do in order to dissolve the salts. Alkalis effect the change one way, and acids effect it the other. Of course, the thing has to be worked out in fairly close detail in order to get the results that are desired; but one can be turned into the other in that way.

THE PRESIDENT expressed the thanks of the meeting to Dr. Levy, after which Mr. G. G. Blake read a paper and showed lantern slides on "Further Notes on Localisation." There was no discussion. A vote of thanks was accorded.

FURTHER NOTES ON LOCALISATION.

By G. G. BLAKE.

We have heard so much about "localisation" lately, that I feel some hesitation in giving another paper on the subject, and will therefore try and make my remarks as brief as possible.

Since I gave a paper before this Society last January, I have extended the application of my method so that it can now be employed either as a "screen" or photographic method, with the tube below the couch and the plate above.

I have prepared one or two lantern slides to illustrate.

In Fig. 1, C represents the top of an X-ray couch. If made of canvas it is as well to place a thin piece of board on the top of the couch on which the two blocks of wood, B and B1, can stand firmly.

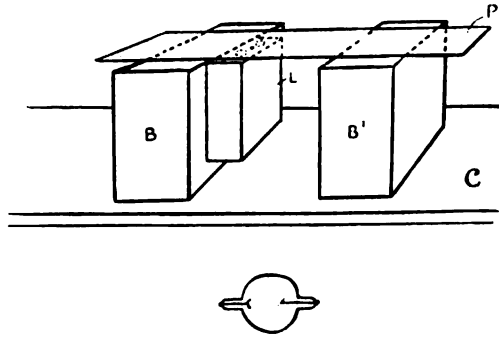


Fig. 1.

The localiser L, Fig. 2, consists simply of a block of wood in which are imbedded two small metal cylinders, T and T1, and a short metal rod or needle N of known length.

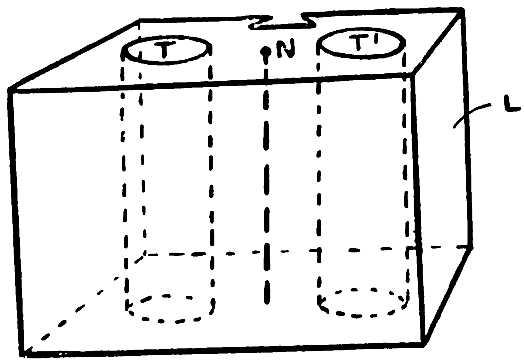


Fig. 2.

The localiser has a groove down one of its sides so that it can be slipped on to a runner on the side of block B (see Fig. 3), and thus held up in position so that the ends of the cylinders and needle are in contact

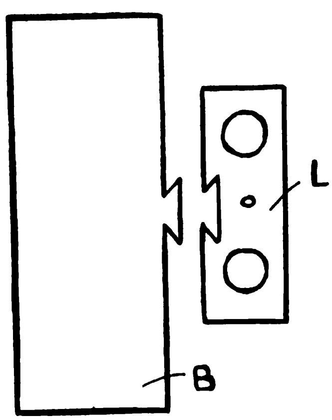


Fig. 3.

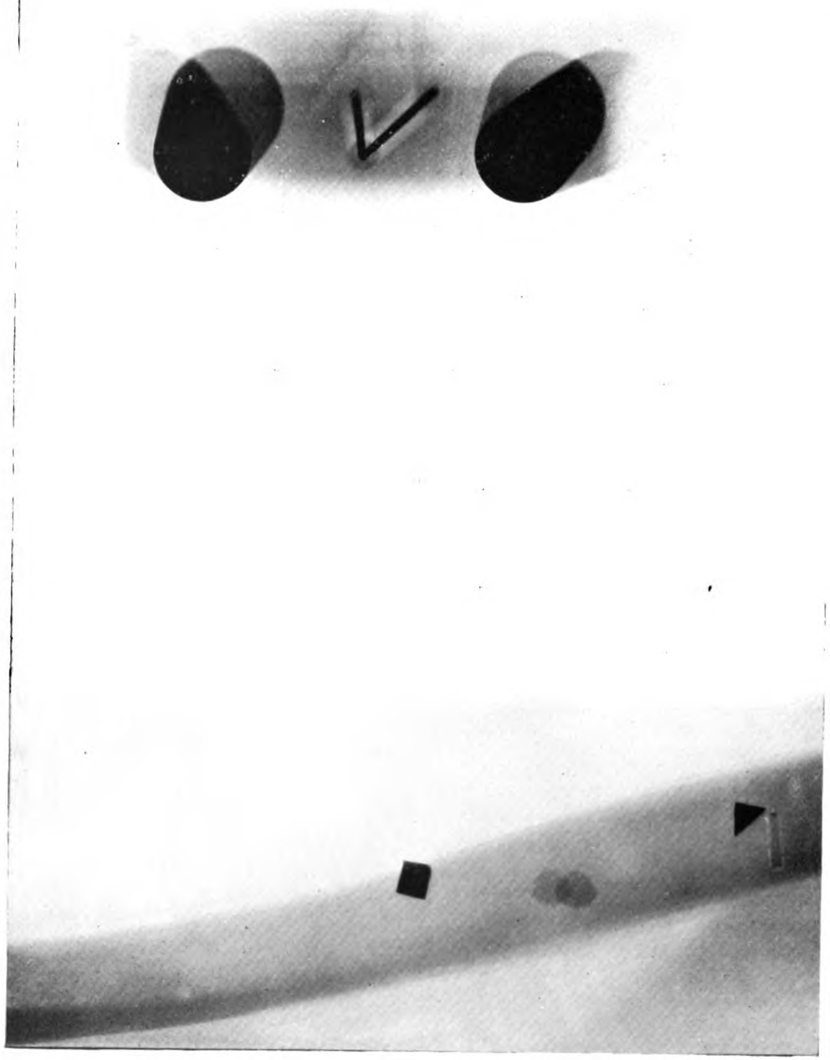


Fig. 1.

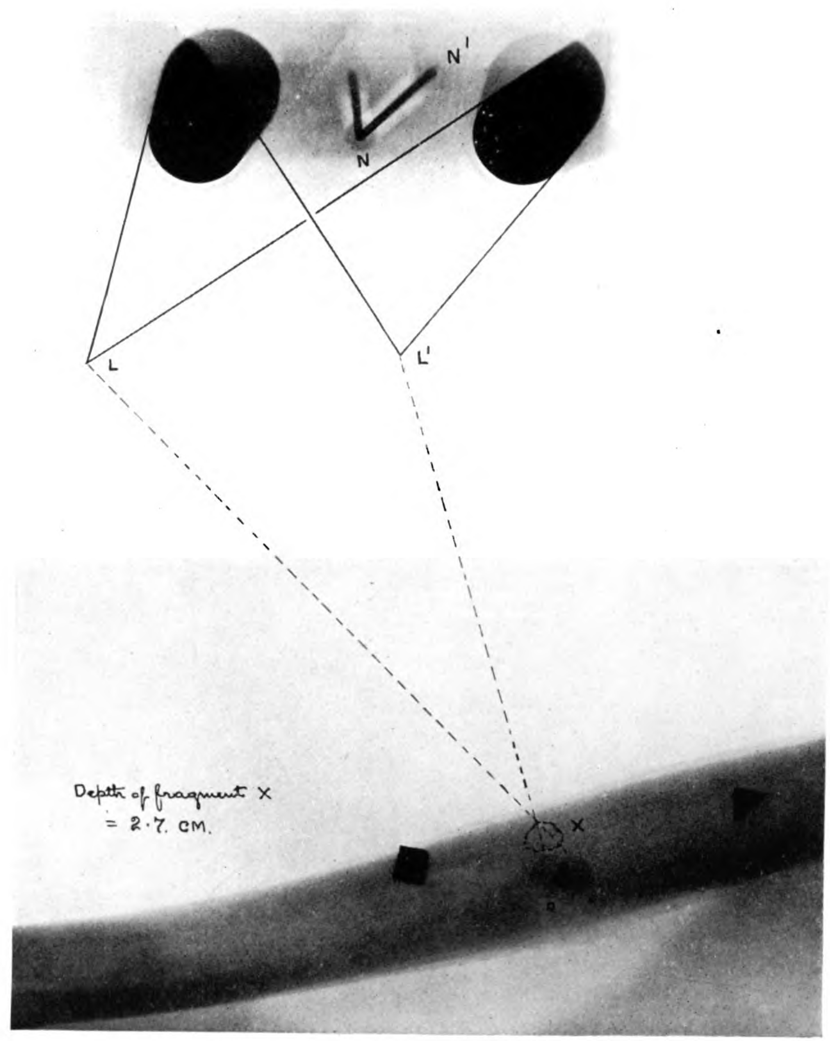
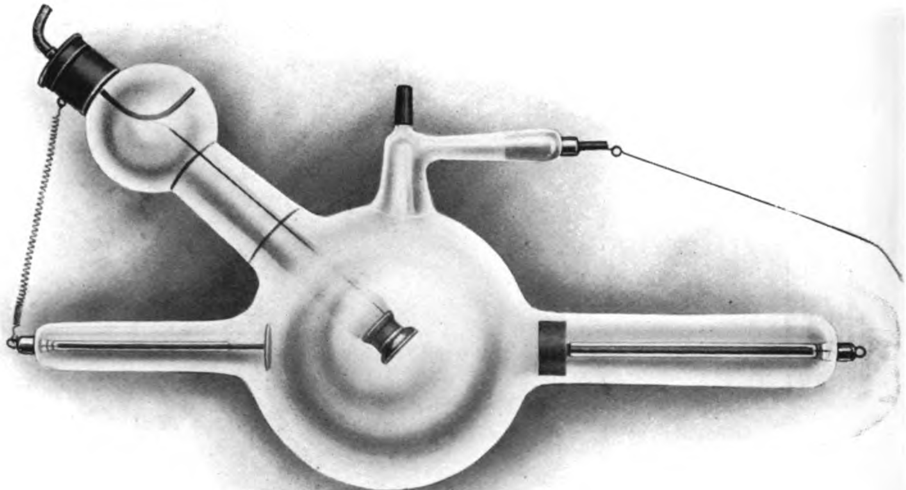


Fig. 2.

To illustrate method of Localisation.—By G. G. BLAKE.

Plate IV.

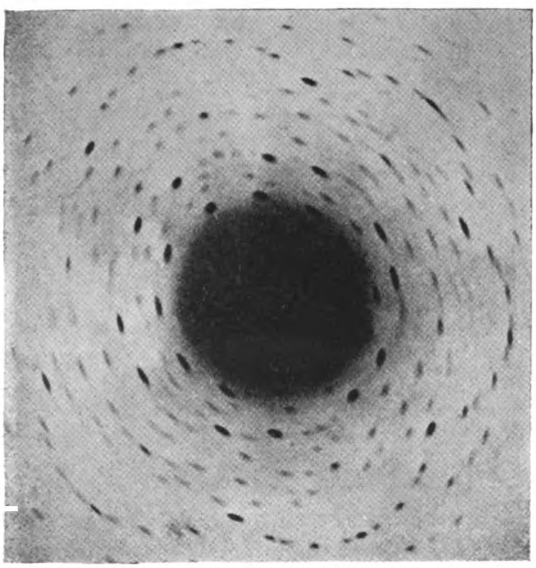
'adiant Matter Tube devised by
William Crookes with which the
iscovery of X-rays was made.



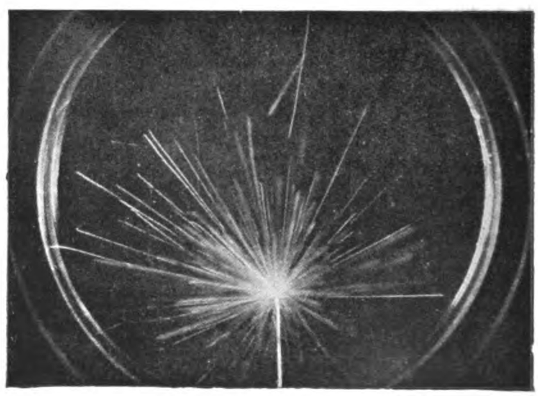
Modern X-ray Tube.

Radiograph made by A. A. Campbell Swinton, F R.S.,
on January, 18th, 1896. *Exposure 4 minutes.*

Thorax radiographed by Dr. Robert Knox in 1915.
Exposure about 1/100 of a second.



Diffraction of X-rays by a Crystal of Beryl showing the regularity
of Crystal Structure. By Messrs. Friedrich and Knipping.



Photograph by C. T. R. Wilson, F.R.S., showing the track of
an α particle after expulsion from Radium.

with the underside of a fluorescent screen P, Fig. 1 (or the film side of an X-Ray plate), when the latter is placed across the top of blocks B and B1.

The limb or other part of the patient to be radiographed is now placed between the blocks B and B1, and is packed up by means of cushions (made of black sateen filled with cotton wool which are transparent to the rays) so that the upper surface of the patient's limb just stands about ½ inch above the level of the blocks, so that when the screen is placed above it, and pressed down flat on to them the limb sinks down into the cushions and is held in contact with the screen.

A screen examination is now made to discover the whereabouts of the foreign body or bodies, the screen removed, and two small and differently shaped markers, made of adhesive plaster backed with lead foil, are stuck on to the patient's skin in any position where they will come in contact with the screen when it is replaced, care being taken that neither of them obscures the shadows of any of the foreign bodies.

The screen is then replaced for a moment to make sure of this, and also to find two suitable positions for the X-ray tube below the couch from which to make two exposures. Any two positions, approximately three or four inches apart, will do so long as the shadows of the height-finding needle and the two cylinders of the localiser L fall on the screen.

The screen is now removed, replaced by an X-ray plate, and the two exposures made.

Plate IV. This shows how the photographs look when finished. The photograph on the left hand side is a print from a negative taken for the Richmond Red Cross Hospital.

On the top of the plate the shadows of the two cylinders and the height-finding needle are seen. The shadows of the cylinders rarely show in their entirety, but one always obtains a distinct dark patch where they overlap. Two shadows of the piece of shrapnel can be seen, and also the two markers, one being cut triangular, and the other square in this case.

To find the exact position of the foreign body, lines are drawn down the "dark patches" already referred to (as shown in the right hand picture, which is a print from the same negative) till they intersect at points L and L1. These points show us the exact position below which the tube was when each exposure was made, and the distance between them is that of the displacement of the tube. A line is drawn from each of the shadows of the foreign body to points L and L1, and the intersection of these lines with each other indicates its true position in relation to the markers (that is of the foreign body).

A tracing on a sheet of stiff transparent celluloid, traced from the back of the negative or from a print, enables the surgeon to find the exact spot at which to make an incision.

Full details of this idea, and also the method of calculating the depth of the bullet was described in a paper which I read before this Society in January last.

When it is desired to localise in the thicker parts of the body, larger blocks are used, or the same blocks can be supported on others to make them up to the required height, or of course an adjustable rackwork arrangement could easily be made to use in their place.

When it is desired to use this as a screen method only, instead of taking a plate, a sheet of thin transparent celluloid is laid on the surface of the X-ray screen, and all the necessary shadows and lines from two positions of the X-ray tube are traced directly on it.

For the finding of the depth of the foreign body from a photograph where the displacement of the shadows of the foreign body is very small, I have found the following proportional method very useful. It is based on the same principle as that which I employ for finding the height of the tube.

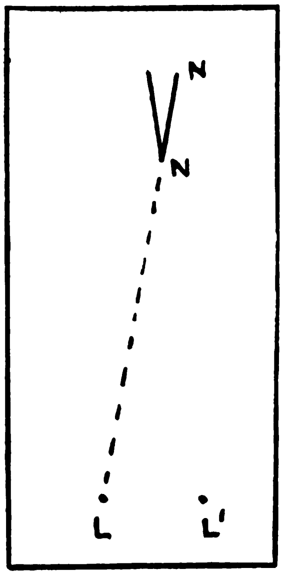


Fig. 4.

Fig. 4 is a diagram where L and L^1 represent the two points above which (or below) the X-ray tube stood, point N the base of the height-finding needle, and N N^1 its shadow (when the tube was above point L).

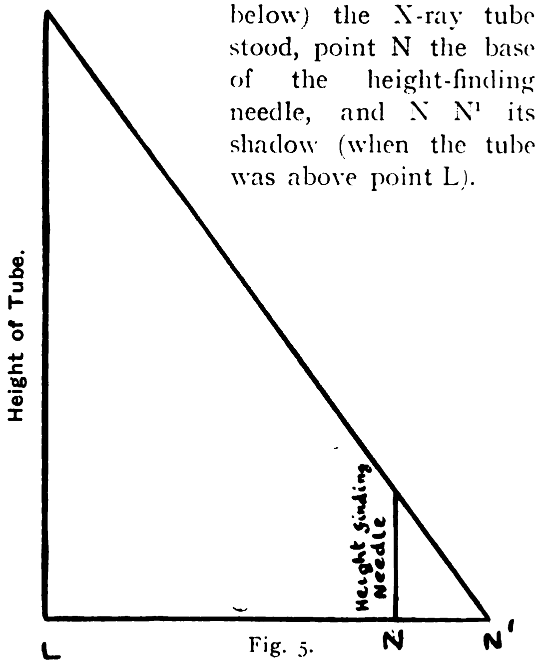


Fig. 5.

From figures 4 and 5 it will be seen that distance N N^1: length of height-finding needle : : N L^1: height of tube.

therefore $\dfrac{\text{height of localising needle} \times N^1 L}{N^1 N}$

= height of tube.

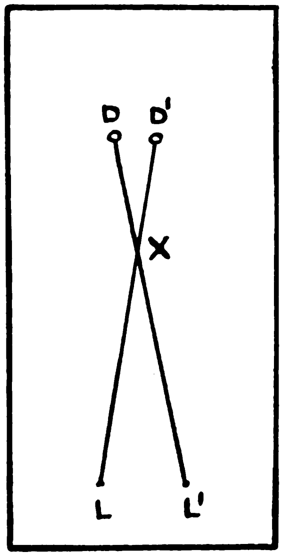


Fig. 6.

Fig. 6 is a diagram representing a finished print or negative showing points L and L,

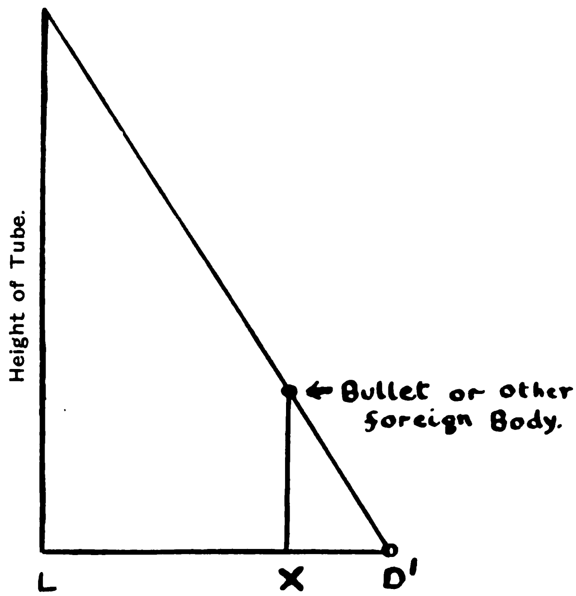


Fig. 7.

the exact position of the foreign body, (bullet) X, and two shadows of the foreign body D and D^1.

From figures 6 and 7 it will be seen that D^1 L: height of tube : : D^1 X: depth of bullet.

therefore $\dfrac{\text{height of tube} \times D^1 X}{D^1 L}$ = depth of bullet.

LETTER TO THE EDITOR.

SIR,

The results of a preliminary investigation which I made upon the X-radiation emitted by a Coolidge tube, were communicated at an Ordinary Meeting of the Society in February last and published in the JOURNAL in the following April (No. 43, Vol. XI.). This communication contains the following statement :—

"There are two features of interest which may be noted :—

(1) A large increase in the yield of X-rays produced by a small increase in the healing current.

(2) A relatively larger yield of 'hard' X-rays than of 'soft' rays when the heating current is increased."

In the Philosophical Magazine for September of this year, there appeared an exhaustive paper on the subject of the Coolidge radiation by Sir E. Rutherford, Professor T. Barnes and Mr. H. Richardson. In the section of their paper which is concerned with the yield of X-rays from the Coolidge tube under various conditions (p. 350) they question the validity of conclusion (2) above and remark upon it as follows :—"In our preliminary examination of this point results were obtained in general agreement with those of Russ, but the differences between the absorption curves was finally traced to another cause. When working with very intense radiations, it was necessary to nearly close the opening in the ionisation vessel by means of the lead slides. Some radiation was found to enter the vessel by scattering from one lead plate to another, or by the excitation of characteristic radiations. When the front of the ionisation vessel was covered with a thick lead sheet, and the rays allowed to enter through a small opening, the disturbance was eliminated and the absorption curves were found to be independent of the current through the bulb over a wide range."

I desire to state that I consider the above objections to conclusion (2) are entirely valid and that the experimental conditions under which the results were obtained which led to this conclusion (2) were such as have been detailed by Sir E. Rutherford, Professor Barnes and Mr. Richardson.

The following statement concerning the X-radiation from a Coolidge tube would, in view of their researches, probably be permissible :—

The proportion of "hard" X-rays to that of "soft" rays, for any specified voltage between the terminals, remains the same over a wide range of values of the heating current.

I am,

Yours faithfully,

SIDNEY RUSS,

Middlesex Hospital.

OBITUARY.

We regret to announce the death of Dr. William A. Malcolm, which took place quite recently. Dr. Malcolm took the degrees of M.B., C.M., Edinburgh, in 1883, and since then has held many appointments in London where he had an extensive practice. For some years past he has taken great interest in electro-medical work and was well-known to the members of the Society. Upon the war breaking out he joined the R.A.M.C. (T), and was sent to the Dardanelles where he contracted dysentery and died in hospital.

ADDRESS OF THE PRESIDENT, SIR WILLIAM CROOKES, O.M., AT THE ANNIVERSARY MEETING OF THE ROYAL SOCIETY ON NOVEMBER 30TH, 1915.

We meet to-day in circumstances of unparalleled gravity. I am sure we are all deeply conscious of the imperative necessity of modifying the methods of our activities, correcting mistakes, and planning reforms without which we cannot hope to maintain our position among the nations. England, our England, is passing through a fiery furnace of stress and discipline, and we must face without flinching the bitter lessons to be learned.

The Nation's attitude towards Science is, I think, largely due to the popular idea that Science is a kind of hobby followed by a certain class of people, instead of the materialisation of the desire experienced in various degrees by every thinking person to learn something about innumerable natural phenomena still unsolved; and having learned, to control and apply them intelligently for the benefit of the human race. Many attempts have been made to explain exactly what is meant by Science and to differentiate true Science from its counterfeit; and it is by no means easy to define it so that the vague general idea of the average man can be replaced by clear and precise conception. Even the most patient investigator, the most acute observer, must constantly feel "Oh, what a dusty answer gets the soul when hot for certainties in this our life." If we refer to our Charter, we shall find that the aim of the Royal Society is promoting Natural Knowledge by Experiments, and if we regard Science as synonymous with Natural Philosophy we may describe it as knowledge relating to natural objects and phenomena connected therewith based upon experiments. Life has been defined as the Act of Correspondence with our environment, and Science may equally tersely be defined as the use of intelligence in effecting that correspondence.

I believe that the "Hobby" attitude is due to our national character, and can only be rectified slowly step by step. We cannot suddenly become a truly Scientific Nation, either now during the War, or immediately on its conclusion. We shall have to make many fundamental alterations in our ideas and almost to change our natures before such a change can be effected. First among our defects must surely be placed mental inertia, our reluctance to do our thinking for ourselves and the slowness of our intellectual apprehension. This condition is fundamentally different from docility of mind, and its results are more disastrous because it tends to inhibit action on the part of those who should be leaders. Associated with it of course is our inherent stolid conservatism, which makes us too readily satisfied to continue in the ways of our forefathers—ways which, though good enough once upon a time are now obsolete and undesirable. We are sometimes prone to under estimate our opponents' abilities and powers, and usually we have a hearty contempt for outside criticisms of our methods. Our mental inertia makes us slow to put our latent organising power into action.

The problem before us is twofold. We have, firstly, to find out how best to organise all our present forces and employ the material at our disposal to win victory. Many suggestions have recently been made as to the best way to mobilise Science and Invention, so that for example, Schemes that show some likelihood of having military or naval value can be put at once to the test. At the beginning of the War the Royal Society appointed Committees for this purpose. Their scope could be extended usefully. They include men of Naval and Military experience, whose practical skill and knowledge supplement the theories of men of Science. The second part of the problem is closely interwoven with the first, and its importance to the Nation is hardly inferior. If we neglect to alter our ways, if we continue to disregard the value of scientific work and are content with ignorance of scientific methods on the part of the Authorities. we shall assuredly suffer total defeat in the Industrial War which must of necessity follow upon the conflict of arms now raging. This is a matter in which men of Science have a great responsibility to the Nation. We must not cease to bring to the notice of the public the facts of which we are too fully aware. The attitude of the Government and the Public towards Science has been mistaken. For this formidable error we suffer and, I fear, must long continue to suffer. The remedy involves many sacrifices and heavy expenditure, probably at first without apparent return. It is to the new generation now being educated that we must look for betterment of our position; and it is for Youth we must now make plans. We must make all education more Scientific. It is admitted we have much to learn from our adversaries; we must bring Scientific methods to the front. As a well known writer has said of our young generation, "We must not let their Schooling interfere with their Education." I am, however, glad to note that already there are signs on the part of some of our larger Companies and more intelligent Manufacturers of a disposition to remedy shortcomings. The numerous "Polytechnics" that spring up in every manufacturing town (some wonderfully well equipped and organised) are turning out men with at least an insight into the scientific principles that underlie their particular spheres of work, and such men find their services readily accepted. There are also within my knowledge many instances where Manufacturers encourage their lads to attend these Institutions, giving them the necessary time and opportunity. But so far this is the isolated action of a few individuals, and needs both encouragement and organisation.

Should not Science be represented on the Privy Council? It is astonishing that in so august a body Science is almost ignored. Ought we not to have in the Cabinet a Minister of Science with a board of advisers similar to that of Agriculture, with the proviso that the Minister of Science should hold his office primarily by virtue of his Scientific capacity? Power of organisation and general business ability should be regarded as essential secondary qualifications. The newly appointed Science Councils and Committees might be incorporated under the Ministry of Science—then and then only pure Research would begin to take its place as an invaluable Profession, with a status of its own at least on a level with that of other learned Professions. The leaders of its rank and file would be doing work of fully as much value to the Nation as the work of the Officers of our Naval and Military forces. Then, I feel convinced, the next generation would see the disappearance of listless co-operation between manufacturer and scientific workmen, and we should hear less of the inferiority of British science as compared with that of our opponents. Given equal opportunities, our men would speedily give proof of fertility of ideas, of organising powers, and of resource and initiative. Research could be so thoroughly well organised that suitable workers would be jointly engaged with those problems for the speedy elucidation of which there is the greatest need, and the results of their investigations would be at the disposal of all British manufacturers. It rests with us to keep these ideas before the mind of the Public now that at last it is ripe to consider them. "Be wise to-day; 'tis madness to defer."

And now I must pass on to my latest task—perhaps the most fateful of all the tasks I have ever undertaken. I bid a sincere regretful farewell to my Official Colleagues of the Royal Society, whose unfailing and courteous help in my discharge of the duties of the Presidential Office I gratefully acknowledge. I deeply appreciate the honour conferred on me during the last two years, and if I may utilise "An Intelligent Appreciation of Events before they Occur."

I heartily congratulate the Society on its election of my successor. We all know, and the world knows, the lofty place held by Sir Joseph J. Thomson in the august realms of Science—and we all must feel that our Society could not have selected a more suitable and distinguished President.

NEW BOOK.

RADIUM X-RAYS AND THE LIVING CELL, WITH PHYSICAL INTRODUCTION. By *Hector A. Colwell, M.B. (Lond.), D.P.H. (Oxford.) Late Assistant in the Cancer Research Laboratories, Middlesex Hospital;* and *Sidney Russ, D.Sc. (Lond.), Physicist to the Middlesex Hospital.* London: G. Bell & Sons, Ltd.

The object of this book is to describe some of the main experimental facts which have been established, as to the effects that the X-rays and the rays from radium have upon living cells.

In order to make the subject generally intelligible, a description of the properties of these radiations was necessary, and Part I. is designed, not only to provide information in this respect, but to meet the needs of those who approach the subject with a view to experimental investigation. A large amount of interest centres around the action which the rays have upon malignant cells, and some of the most detailed studies have been made by investigators in this connection. The results which have so far been reached are, one may venture to hope, a good augury for the foundations of a rational basis of radio-therapy. This, however, is but one aspect of the effect of the rays upon the living cell, and the authors believe that an increased knowledge of the reactions exhibited by the normal healthy cells and tissues is necessary before the real nature of the processes set up by these rays is revealed.

The subject is not approached from the clinical aspect, but data have frequently been selected from the details of clinical observations, when these have borne upon the subject-matter in question.

The foregoing preface outlines the scope of the latest and most complete treatise dealing with the effects of irradiation upon the living cell. The names of both authors are well known to the readers of this JOURNAL, and they are to be congratulated upon the results of their labour; the book shows a very considerable advance upon true scientific lines. Its publication at the present time, when there is a marked general disinclination to great effort in any direction other than that connected with the war, is a very happy indication of that latent vitality which we are persuaded is still undiminished for all the present stress.

A copy of the work has been presented to the Society and can be consulted, but Members are reminded that, according to the rule heading the library list in our last issue, "books cannot be removed from the library."

ABSTRACTS.

The following are selected from the current numbers of "SCIENCE ABSTRACTS" *as likely to be of special interest to members of the Society, and are published by permission of the Editors of that Journal.*

1427. *Photographic Spectro-photometry of X-rays.* W. FRIEDRICH and P. P. KOCH. (Ann. d. Physik, 45, 3, pp. 399-418, Sept. 15, 1914.)—The authors give an account of their attempts to develop a method of determining relative X-ray intensities by other means than the usual ionisation methods. A method making use of photographic photometry is worked out, and the results of the authors' experiments lead them to give the following as the most satisfactory procedure:—After one has made certain, by means of a test, that the highest intensity to be measured gives a blackening that is still within the range of blackening obtainable with the tube photometer, a plate is exposed in the photometer, simultaneously and for the same length of time as for the measuring plate (the relative intensities of an image on which are to be measured); this first plate is the photometric plate with the intensity scale. The X-ray tube which is supplying the rays for the measuring plate is caused to play upon a metal sheet of suitable substance, which latter yields a secondary radiation of hardness somewhat less than that used for the exposure of the measuring plate; this photometer being kept in rotation during the exposure. The measuring and photometric plates are then simultaneously developed, fixed and washed. Measurement of the blackening can be made with any photometer or any suitable scale. In cases where a simultaneous photometric scale is not made, the relative intensities can be estimated within 10 per cent. by using a formula and tables for use therewith which is given in the paper. L. H. W.

1578.—*Experiments on the Production of Helium.* A. DEBIERNE. (Ann. Physique [IX], 2, pp. 478-488, Nov.-Dec., 1914.)—This paper deals with some experiments on the production of helium by other than radioactive agencies. Certain dark-violet fluorspars (almost black sometimes) smell of ozone. When they are heated, they lose their colour and also their former thermo-luminescence. All these specimens yield helium on heating, in very variable but always small quantities from a few tenths of a mm.^{3} to 50 mm.^{3}). On exposing such a specimen, which had apparently given up all its helium, to radium rays, it turned violet again, and renewed heating then once more revealed the helium spectrum; but a second test of this kind did not give helium once more. In other experiments electrolytic gas was continuously being burnt for days, without

explosions, by being kept in contact with heated copper and copper oxide; not a trace of helium was ever found in this gas, nor in electrolytic gas produced by decomposing water by radium rays. In other experiments the disruptive discharge of a mercury-arc was continuously sent through the vapour: the mercury in the one end of the glass tube was in constant communication with a mercury reservoir to maintain constant conditions for days. No helium was ever found: nor was any discovered when oxygen was introduced into the apparatus, and the arc worked again. H. B.

1579.—*Gases produced by Radio-active Substances. Decomposition of Water.* A. DEBIERNE. (Ann. Physique [IX], 2, pp. 97-127, Aug. 1914.) – Reviewing the literature on the subject, the author concludes that all the three kinds of rays, α, β, γ, participate in effecting decomposition, and that the decomposition takes place in three ways: by direct collision of the charged particles with the molecules of water: by the local rise of temperature along the path of a particle; by the secondary electrolytic phenomena due to the ionisation, which are important even with α-rays. The interaction of the ions, which are designated $\overset{+}{H_2O}$ and $\overset{-}{H_2O}$, with the ions in the water (hydrogen and hydroxyl) liberates hydrogen, hydrogen peroxide, and oxygen. The relative share of the different particles and the importance of the several reactions are not certain; β-rays, however, give nearly pure hydrogen. In the author's new experiments the radium salt was enclosed in a thin-walled glass bottle and placed within the cavity of the U-shaped glass cell so that the rays had to traverse two glass thicknesses before reaching the water in the cell. Assuming that the number of ions formed in the water is the same as would be produced in a gas, the calculated and observed rates of decomposition are of the same order. H. B.

1415.—*X-ray Spectra of some Metals.* H. ROHMANN. (Phys. Zeits, 15. pp. 715-717, July 15, 1914.)—The author gives a series of reproduced photographs of the X-ray spectra of the metals Ni, Cu, Zn, Mo, Ag, Pt, Au, Th: the metals being used as antikathodes in a small X-ray bulb:—Thorium shows a remarkable behaviour when exposed to kathode rays. If after the bulb has been some time in use the antikathode is shifted a little so that the focus now falls on a new spot, the spot which previously was the focus lights up, under the influence of the weak radiation, with a very intense bluish fluorescence—much stronger than the focus itself. This effect continues for some time. L. H. W.

1416. — *Measurement of X-ray Energies.* B. WINAWER and ST. SACHS. Phys. Zeits. 16. pp. 258-264, July 15, 1915.)—A description is given of a simple method of measuring the energy of X-rays. The principle of the method is as follows: The X-radiation is compared with the γ-radiation from radium, a thin-walled "air-electroscope" being employed for this purpose. From the ratio of the observed ionisation currents, the known absorption-coefficient of γ-rays in air [see Chadwick, Abs. 1200 (1912)], and the coefficient for X-rays (easily determined from observations of the absorption of X-rays in water or in liquid air), it is easy to obtain the "*true*" radium-equivalent of the X-radiation. The observations made with the "air-electroscope" can be used in the calculation of a reduction factor for any simple metal electroscope of the ordinary type. When such a factor has once been determined, it is then possible to use the ordinary electroscope for the direct comparison of the energies of γ-rays and X-rays.

The radium-equivalents of the X-rays thus determined are in good agreement with the calculated heating-effects. As a consequence of measurements of the above character, the authors suggest a *unit of energy to be used in X-ray measurements.* It is defined in the following way:—A beam of X-rays will have unit energy if, by its complete absorption in air, it produces the same number of ions as the γ-rays from 1 gm. of radium (B + C) would produce under similar conditions.

A. B. W.

1419.—*X-ray Wave-lengths.* W. DUANE and F. L. HUNT. (Phys. Rev. 6. pp. 166-171, Aug., 1915. Abstract of paper read before the Am. Physical Soc., April, 1915.) Using a Coolidge tube with a tungsten target, it is shown that a *constant* p.d. produces X-rays of a great variety of wave-lengths. Energy measurements were made for 6 different wave-lengths, corresponding to 6 different "glancing angles" respectively. From observations of the voltage applied to the tube, the ionisation current and the current passing through the tube, the authors calculate the value of "h," Planck's constant from the relation $V_0e = h\nu_0 = hc/\lambda_0$, where e = electron charge, c = velocity of light, and V_0 is the minimum voltage required to produce X-rays of wave-length λ_0. The average value h determined in this way is $6{\cdot}39 \times 10^{-27}$, in close agreement with the value $6{\cdot}41 \times 10^{-27}$ given by Planck. The results thus give strong support in favour of the fundamental principle of the quantum hypothesis, for they show that the rays of frequency ν are not produced unless the energy available is equal to $h\nu$. It is also shown that the ratio of λ_e to λ_0 where λ is the effective wave-length of the whole radiation and λ_0 is as defined above, is about 1·50 at 40,000 volts and after the rays have passed through 3 mm. of Al. This ratio increases as the voltage increases.

FIG. 1.

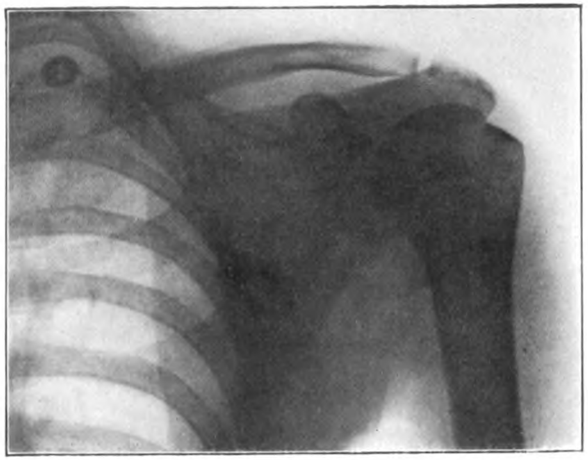


FIG. 2.

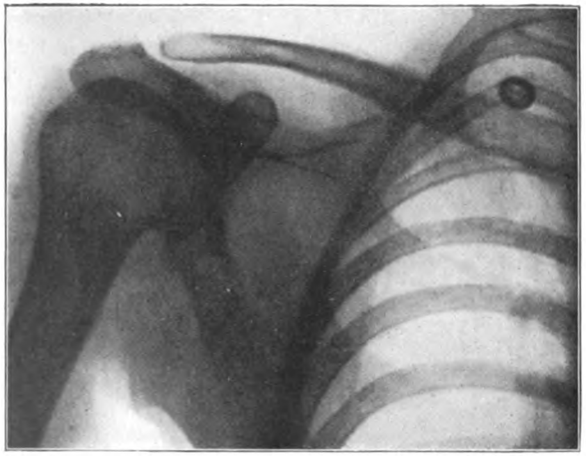


FIG. 3.

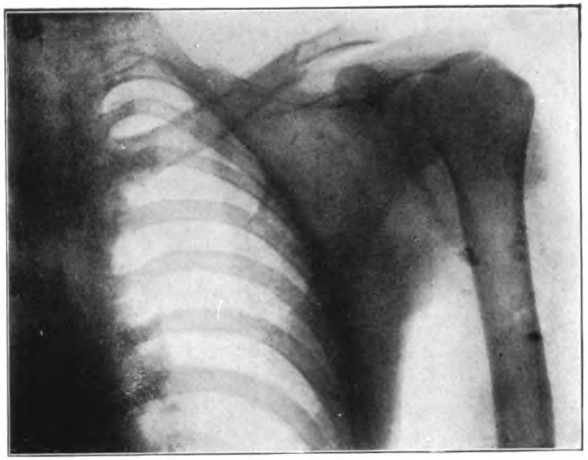


FIG. 4.

By LEONARD A. LEVY, M.A. (Cantab.), D.Sc, (Lond.), F.I.C., F.C.S.

PLATE II.

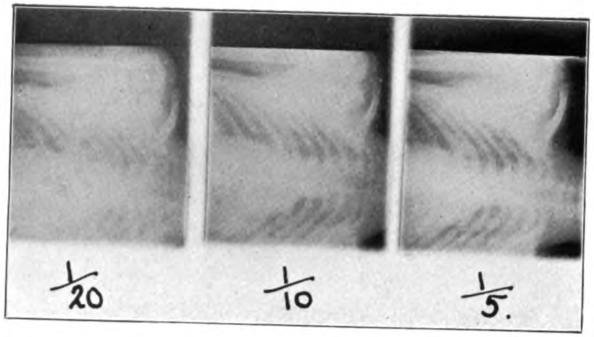


Fig. 5.

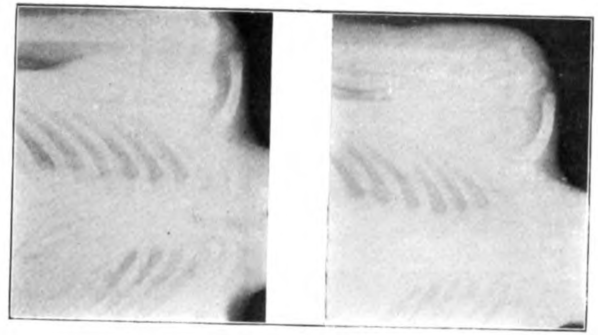


Fig. 6.

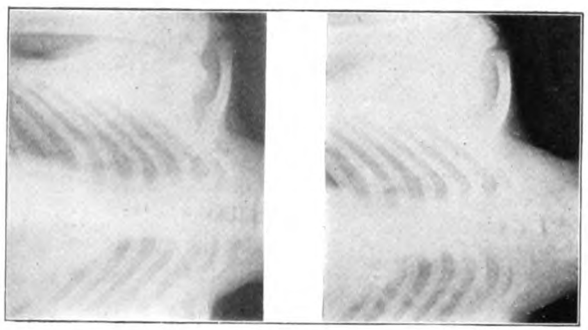


Fig. 7.

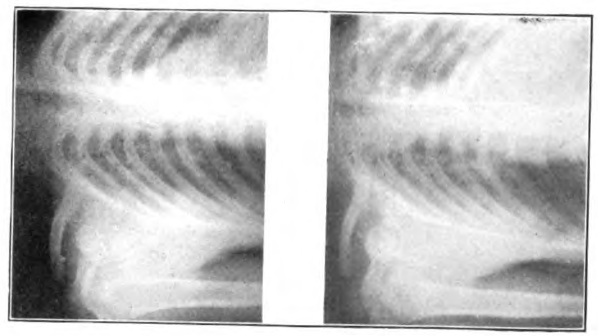


Fig. 8.

By LEONARD A. LEVY, M.A., (Cantab.), D.Sc. (Lond.), F.I.C., F.C.S.

Plate III.

Price to Non-Members,
4s. *net*; 16s. *per ann.*

THE JOURNAL

OF THE

RÖNTGEN SOCIETY.

VOL. XII. APRIL, 1916. No. 47.

ORDINARY GENERAL MEETING.

AN ORDINARY MEETING of the Society was held at the Institution of Electrical Engineers on Tuesday, January 4th, 1916, Mr. J. H. Gardiner, F.C.S., President, in the chair.

The minutes of the previous meeting were read and confirmed.

Nominations, approved by the Council, for ballot at the next meeting :—

Dr. C. H. BROOKES, M.R.C.S., 2, Fitzjohn Avenue, Hampstead, N.

Proposed by Robert Knox.
Seconded by Dr. Harvey.

JOSEPH E. BARNARD, Park View, Brondesbury Park, N.

Proposed by G. H. Rodman.
Seconded by Robert Knox.

Miss ALICE M. ASHWIN, Waterloo Hill, Stratford-on-Avon.

Proposed by Robert Knox.
Seconded by F. Harrison Glew.

Mr. T. CLARKE, 96, Mildmay Road, N.

Proposed by Dr. Cumberbatch.
Seconded by Robert Knox.

Mr. CHARLES EDWARD LAWRENCE, 196, Great Portland Street, W.

Proposed by Geoffrey Pearce.
Seconded by Robert Knox.

Dr. R. LEDOUX LEBARD, Chef de Centre de Radiologie de la 9th Region, Tours, France.

Proposed by J. H. Gardiner.
Seconded by Robert Knox.

The following were elected members of the Society by show of hands :—

H. T. GEORGE, B.A., M.R.C.S., L.R.C.P., 33, Ampthill Square, N.W.

ARTHUR C. G. BEACH, 49, Park Road, West Dulwich.

THOMAS THORNE BAKER, F.C.S., Fairleigh, Wareham Road, South Croydon.

Mr. Campbell Swinton then took the chair, while **Mr. J. H. GARDINER** read the following paper, illustrated by lantern views, and specimens of minerals.

NOTE ON THE OCCURRENCE OF URANINITE (PITCHBLENDE).

By J. H. GARDINER, F.C.S.

Pitchblende, the mineral from which our supply of Uranium compounds has been obtained, is a black, pitch-like substance with a density of from 5·0 to 6·5. It occurs both as a primary and a secondary constituent of rocks, and has been found in widely separated parts of the earth, Norway, Bohemia, America, Cornwall, and other localities. The primary form is crystalline, the crystals belonging to the cubic system, generally octahedra and dodecahedra, with a density of

from 8·0 to 9·0, and is known under the names Broggerite, Cleveite, Nivenite, etc. In these varieties the uranium has been partly replaced by the rare earths and thoria.

The most important variety, however, is the secondary massive and hydrated form ; this has been found in Saxony, America, Cornwall and in the celebrated mines of St. Joachimsthal in Bohemia ; these latter deposits have been worked for many years, and have supplied practically the whole of the commercial uranium compounds. Here it has been the practice to extract the crude uranium on the spot, and large quantities of residues had accumulated.

When the discovery of radium was made, these residues were found to be very rich in that valuable substance, and it is from this source that the greater part of the radium now in use has been obtained. Another source of uranium has recently come into prominence and will probably yield a good supply of radium. I refer to the recently discovered mineral Carnotite, a vanadate of uranium and potassium carrying about 60% of uranium ; this has been found in America and in South Australia.

The amount of radium in radio-active minerals is in proportion to the uranium content, and is given by Rutherford and Boltwood as about 3·4 parts in 10,000,000 of uranium element.

Both the terms pitchblende and carnotite are somewhat indefinite ; material may be legitimately called pitchblende in which the $U_3 O_8$ content may be anything between 10% and 80%. A specimen that I have here purchased a few years ago in Germany as carnotite contains less than 20% uranium, and analyses have been published of American ores containing less than 2% uranium.

As is well-known to most here, the degree of radio-activity in a mineral can be determined quickly and with certainty by electrical methods, but if one's object is not so much the value of an ore by the radium that it contains, but to observe the manner in which its active constituents are distributed, then obviously the photographic property possessed by these bodies is the more suitable.

If we lay a piece of pitchblende upon a photographic plate and leave it for a few hours, we get, upon development, an accurate record of the distribution of the radio-active material throughout the mass ; if the surface of the mineral is carefully flattened so as to come into optical contact with the sensitive film it is possible to obtain radiographs of very great beauty and detail, in which every concentration and distribution of the active material is shown. My object to-night is to bring before you a few of such photographic records, veritable natural "manuscripts."

I may say at once that if we examine specimens of pitchblende from different localities by this means, it becomes apparent that there are peculiarities in the records that are common to each source ; the ore from St. Joachimsthal, for instance, is quite different from that from Cornwall, and this again from other deposits, that is as far as I have been able to observe from the specimens at my disposal.

Taking first the St. Joachimsthal ore, which is exceedingly rich in uranium, Plate IV., Fig. 1 is a radiograph from a specimen that was sold as pitchblende ; you will notice that quite a large portion of it is practically inactive rock, of no value at all, but you will also notice some curious "markings" in the active portion shown in the blackened area to which I want to draw your attention.

You will notice in the first place that the active surface is traversed in several directions by fissures or cracks of different widths ; these fissures are characteristic of secondary deposits of pitchblende from all localities and appear to be due to some kind of shrinkage, since the material was originally deposited.

In addition to these cracks or fissures, there are seen some thick white lines that cross the surface, these are due to the intrusion of layers of soft inactive matter which sometimes crumbles out during the flattening process, and lastly you will notice that the whole surface presents a network of fine lines of inactive matter, examined under the microscope the cause of

these fine lines in the specimen appears to be due to extremely thin deposits of crystalline sulphides, probably of iron or copper. A close examination brings out the curious fact that distributed through the block there are several areas of comparative inactivity, and each of these areas of inactive material are completely encircled with a white line; had I wished to draw your attention to these portions, and had traced them round with a fine pen, I could not have done so with any approach to the minute perfection that we see in this natural writing; what it means is hard to understand, for we know so little of the language, we stand here upon the borderland between the known and the unknown.

When we come to examine in the same manner the uranium minerals from Cornwall, striking differences are at once apparent; among the many inactive materials that accompany the uranium oxides in this locality the sulphides of copper and iron are very prominent, and they occur, not in thin streaks and lacings such as we have been considering, but in large masses, often in well-formed crystals, embedded in the radio-active material. Before entering upon the description of these Cornwall specimens, I would like to record my indebtedness to Mr. Arthur Schiff, of the British Radium Corporation, who not only placed at my disposal a number of interesting specimens of minerals selected from the mines at St. Ives, but gave me much information as to the occurrence of the deposits and details of their work. It is a matter of considerable satisfaction to know that we possess in our own country a large amount of material rich in uranium, radium and its accompanying bodies, tin, copper, bismuth, lead and many other elements of value that need only the power of peaceful industry to win for our use and benefit.

Fig. 2 is a radiograph from a very fine specimen of dense black pitchblende from "Trenwith" mine, St. Ives, Cornwall. You will notice several large irregular areas of inactive matter that have left no impression upon the film; these are for the most part masses of metallic sulphides of no particular crystalline character embedded in the active uranium oxide.*

Fig. 3 is interesting, as it shows a very fine cube of FeS_2 introduced into a mass of active matter. The inference is almost irresistible that in this case the uranium compounds were deposited subsequently to the formation of the crystals of pyrites, and that since that time very little if any movement has taken place, for the crystals of $Fe\,S_2$ are very soft. The next two specimens are pieces of common country rock from the waste heaps; Fig. 4 shows that active material has been deposited in very thin layers, one layer well in the rock formation and the other upon the surface; the nebulous cloud that shows here is probably largely due to the alpha particles, which are free to fly off into space from all directions. The specimen Fig. 5 points to a different state of affairs; the radiograph shows that thin strata of uranium compounds have been deposited between layers of the material, which has subsequently been subjected to great stress, converting the thin streaks of active matter into flowing zigzags. These distortions, if I read the writing aright, indicate great geological activity, and in the next specimen, Fig. 6, there appears to have been a perfect tornado of disturbance; the thin strata of active material has been crumpled up into a fantastic tangle of twists and turns. If we consider that "The most formless scratch is a perfect record of the forces which gave rise to it," we cannot avoid speculating upon the conditions that existed in this locality at the time when the deposits were formed.

The photographic effects that I have been describing are in all cases, except in the upper layer of Fig. 4, due to the β and γ rays of radium. This is shown in Fig. 7, which is the same block as Fig. 6, but between the mineral and the sensitive film there was placed a sheet of aluminium sufficiently thick to absorb all the

* The separation of these materials should not be at all difficult by simple mechanical means on account of the greater density of the uranium compounds, in point of fact Mr. Schiff has given me a sample of such concentrates that is almost pure uranium oxide.

alpha particles and some of the lower velocity β rays, but you will notice that except for a haziness due to the extra distance of the material from the film the two radiographs are identical.

The fact that these specimens are continuously throwing off torrents of alpha particles can be demonstrated by means of a Crookes's Spinthariscope having a transparent screen. Using one of these instruments it is easy to trace the radio-active portion of the rock by the showers of scintillations that are produced in their proximity.

The pictures that I have so far referred to are true radiographs, where the photographic action is produced entirely by the radio-active constituents of the specimens; if, however, the minerals are examined by ordinary diffuse light, it is seen that in many parts black material occurs that further examination proves is not uranium. This is shown by the light photograph of a specimen of ore from Trenwith Mine, Cornwall, Plate V., Fig. 8; the lighter portions are layers of Pyrites, the central dark part is active uranium oxide and the dark portion at the bottom corner is quite inactive matter. By reference to the radiograph of this specimen, Fig. 9, the radio active portion can be easily recognised. If, however, the block is illuminated, not by diffuse light, but by a bright point of light placed at a particular angle, it is found that a sort of selective reflection occurs, and a photograph taken under these conditions, Fig. 10, gives a result more closely approximating to the radiograph; if a specimen is taken in the hand and the reflected light is examined through a Nicol's prism at a particular angle the uranium portions of the mass can be distinguished by their velvety black appearance.

Having now traced the manner in which the active uranium material is "mixed up" with the inactive constituents in the secondary deposits of pitchblende, I want to turn for a few minutes to the primary crystalline formation. I am greatly indebted to Mr. Glew for some very fine crystals of Broggerite; this mineral occurs at Anneröid, near Moos, in Norway; the density is considerable, 8·7—9·0, and the composition Ceria 0·4, Yttria 1·4, Thoria 4·7, with traces of $Zr\,O_2$, Th, Pb, Fe, Ca and Si, the remainder is $U_3\,O_8$. These crystals are very active, as can be seen from Fig. 11, which was made by laying the natural crystals on a sensitive film for twenty-four hours and developing; the very fine specimen at the bottom shows traces of the geometrical pattern produced by radiation from the sloping sides of the crystal.

We have seen that except in the case of the first photograph there does not appear to be any special design in the arrangement of the active portions of the mineral with respect to the inactive, that form of the mineral appears to be simply a mechanical mixture; but it was thought possible that in the natural compound, where the force of crystallisation had come in to arrange the heterogeneous collection of active and inactive bodies enumerated above into ordered shape, that some such arrangement might be possible, and that one might find that the active portions had taken some special position in the crystal. I may say at once that no such sorting out has been found; it appears to be a case of a perfectly homogeneous mixture in which the active constituents are uniformly distributed throughout the body of the crystal.

The best crystal in my collection was taken and the case carefully flatted* and a radiograph made.

Fig. 12 shows the very even distribution of the radio-activity. As a still further test a twin was selected where two crystals had grown one into the other, in the hope that some special mark might be found at the junction, but beyond the fact that a fragment of inactive matter had been enclosed the mass again appears to be perfectly uniform, as shown in Fig. 13.

Mr. CAMPBELL SWINTON said that they were most deeply indebted to the President for his very interesting paper. It might be news to many of them—it certainly was so to himself—

* In spite of all care taken, a portion was broken off in the process. The crystals are very fragile.

that a new source of radium (carnotite) apart from pitchblende had been discovered.

A vote of thanks was accorded to Mr. Gardiner by acclamation, and the President then resumed the chair.

EXHIBITS AND DEMONSTRATIONS.

A NEW THERAPEUTIC TUBE STAND.

Mr. P. J. NEATE described the device by a model. He said that as a member of the governing body of the Cancer Hospital at Fulham Road he came across the new therapeutic X-ray localiser elaborated by Dr. Robert Knox and Mr. A. St. George Caulfeild in the radiographic department. This was a device for applying X-rays in a rotary fashion to a growth, and was fully described in the *Archives of Radiology and Electrotherapy* for November, 1915. He (the speaker) had been interested in the contrivance, and the machine, a working model of which he brought before the Society that evening, was simply his own effort, as a mechanician, to develop the idea somewhat further, and to get over certain difficulties. He disavowed all medical and surgical knowledge of the question ; his interest in the matter was merely that of the engineer. His mechanism also involved the principle of the moving tube, carried in such a manner that the rays met the skin surface over a large circular area, so as to cause the minimum of impact at any one place, while yet the whole of them were concentrated at a point below the skin where the influence of the radiation was desired. By means of a long cone, eccentrically mounted on a spinning table, he represented the output of the rays from the rotating tube, and with a board on which was sketched a cross-section of the human body, he was able to demonstrate how the cone of rays could be directed at various angles of inclination into the body, and the effects obtained at various depths in respect to intensity by the widening or narrowing of the cone of rays by stops. For small tumours it was necessary to put in a small diaphragm, and this in the model was represented by the cone of one in twenty-four taper, producing a small double-cone-shaped locus of greatest intensity. The aim was, of course, to direct the X-rays into the interior of the body, and to produce the maximum effect below the surface, while the skin area over the tumour received the minimum of the radiant energy. Mr. Neate then proceeded to explain the construction of his instrument for carrying the tube. In the model, which was on a quarter scale, a little electric lamp was made to represent the tube, and below it was a sliding diaphragm.

Fig. 1.

The tube carrier was attached to and overhung from a horizontal tee-shaped plate, supported at its extremities on three parallel struts pivoted at top and bottom in spherical joints. A pair of Hooke joints prevented angular rotation, an adjustable guide determined the inclination of the struts from vertical to 45°, a belt-pulley gave gyratory movement in a rotary path without actual rotation, and a simple linkage constrained the carrier, and consequently the beam of rays, to keep parallel to the three struts, whatever their movements.

Consequently the beam pivoted on a focal point at the same level as the three lower spherical joints.

A vertically adjustable couch enabled the patient to be placed in such a position that his tumour coincided in location with the focal point of the gyrating beam.

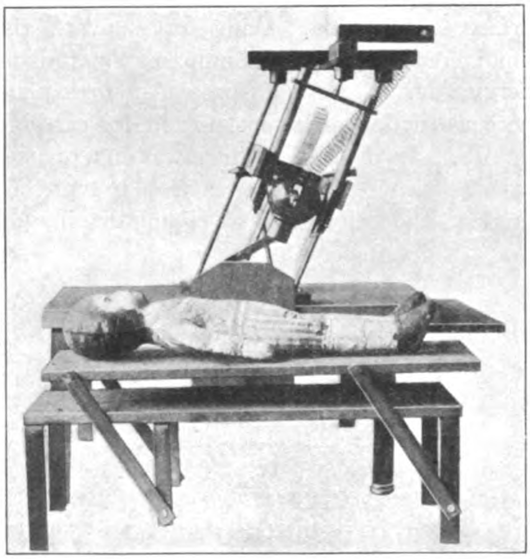


Fig. 2.

By means of a sliding arrangement upwards and downwards, one was enabled to adjust the tube so as to give whatever sort of dose was necessary and desirable at a variable radius. After carrying out the demonstration with the model, Mr. Neate said that he would like to disclaim anything like finality on the subject, but trusted that he had succeeded in making a workable mechanical device.

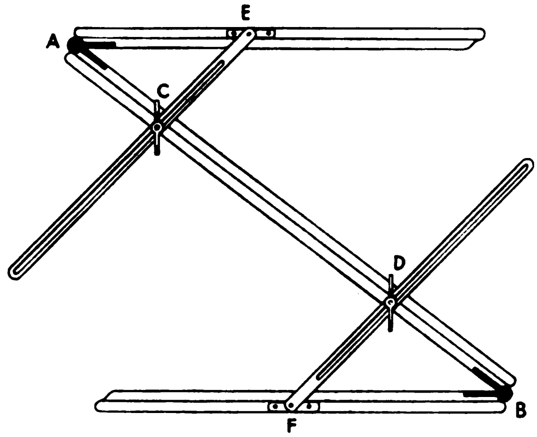
The PRESIDENT expressed the indebtedness of the Society to Mr. Neate. They were especially pleased to see a model of the apparatus, for the advantage of a model was that all the details could be worked out carefully beforehand. Undoubtedly the instrument would be of great utility when it was constructed full size and actually in use. The irradiation of deep-seated cancers was one of the latest developments in the application of X-rays. He had had the pleasure of seeing the other machine to which Mr. Neate had referred, and it worked very beautifully. This one went a step further, and he had no doubt that that, too, would soon be doing good work.

Mr. Neate desires to add that the device has not been patented, and the model can be seen by anyone interested in it, at the Cancer Hospital by obtaining permission from Dr. Knox.

Mr. C. A. SCHUNCK showed a number of lantern slides illustrating bullet injuries to soldiers he had radiographed during the past year at Lady Lindsay's and Swincombe House Hospitals in his neighbourhood, both of which are in connection with the 3rd General Southern Hospital, Oxford. The slides included a compound fracture of the tibia and fibula, a sequestrum in the humerus (Plate VI., Fig. 1), after removal of which the wound healed up well; a fragmented fermur close to the hip (Fig. 2), which has left the limb 3-in. short. This was a case of a sinus that would not heal; and on opening out the limb two cavities, that appear light in the figure, were found full of pus, on removal of which the limb healed up nicely. Fig. 3 shows a bullet injury above the elbow joint with a large piece of metal embedded. This piece of metal must have moved since being first radiographed, when it was stated to be located in the joint itself. A part of a bullet lodged near the left lower jaw-bone is seen in Fig. 4; the bullet entered the cheek on the right side, but did not come out; there was much swelling on the left side and there was difficulty in opening the mouth. Figs. 5 and 6 refer to an interesting case of the shattering of the radius of the left arm. After the damage had been observed by the radiograph, the case was sent to Oxford, where the parts of the bone were brought in line and plated, which is shown in Fig. 6 radiographed three months after Fig. 5. After plating, intense inflammation set in, and the man's life was despaired of for twenty-four hours. Suppuration took place and the plates had to be removed. Fortunately the arm then made a good recovery, and nearly the whole use of it was obtained, so much so that the owner got his commission in the Home Defence Forces.

This appears to be one of those cases of toxic infection that have occurred in bullet injuries in the war. The dormant microbes no doubt became active on the arm being operated upon, and caused inflammation, etc., which is supported by the fact that none of the other cases operated upon that day showed any abnormal after-effect, pointing to the fact that no outside infection took place. Other slides showed injuries to and pieces of metal embedded in the humerus, in the lumbar region, in the foot and the femur. Mr. Schunck also showed a slide (a civil patient) illustrating the growth of bone during four years after a fractured tibia had been plated, evidencing that the osseous tissue had formed to a considerable extent over and around the plate.

Dr. N. S. FINZI exhibited a new form of screen rest. He said that he was a very keen advocate of working with the tube below the couch in the majority of cases, except for stomach examinations and the like. For this purpose one wanted to avoid coming closer to the rays than was necessary, especially with the hands to hold the screen, for even though protective gloves were worn, these were apt to get cracked. He got so tired of using blocks and sandbags and wedges that at length he devised an adjustable block or screen rest, which he brought before the members that evening. The rest was so constructed that it could be put above the patient and pressed down into any required position at any height and at any angle by adjusting the tension of the screws provided. The rest was a hinged Z-shaped piece of wood (or rather three pieces of wood with a couple of hinges), together with slotted guides which were pivoted at one end, and were clamped with a spring washer which worked on the slotted part. Dr. Finzi brought forward a pair of rests which Messrs. Newton had made for him. The device was quite simple and easily made up, and should not be expensive. His colleague, Dr. Eccles, had also devised a similar arrangement, and he would ask that Dr. Eccles might have the opportunity of showing his screen rest to the Society.



At A and B are hinges extending the whole width of the wood. At E and F the slotted guides are pivoted and the fly-nuts at C and D act on screws passing through the slot and compress spring washers on to the guides. A plain brass washer is placed above and another beneath each spring washer.

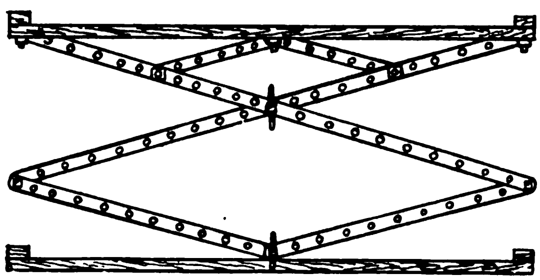
The support when closed is only 4 cm. deep.

Dr. ECCLES, who had been working at the same subject, independently, also showed a similar screen support, which he had devised.

The cantilever principle was adopted, and the device was made of parts of a child's toy, known as the " Meccano."

These can readily be bought at a cheap price, and the whole support easily made by a mechanic in a short time.

The rest can be placed at any angle, and will press down and remain in position, being held by tension thumb-screws and washers.



Dr. JOHN O. HARVEY showed a new form of localiser, the principle of which was well known. It was rather in its practical application that it was in any way new. It had always appeared to him that when the

mechanical principles and methods adopted in our large machine shops in preparing the work for operation were compared with those adopted usually by the Radiographer, there appeared much that was lacking in the methods of the latter. The mechanic used various precision gauges, was constantly checking his work by their means to see it was correct, but as regards skiagraphy in relation to "localisation," the speaker had lately come to the conclusion that with regard to precision it was by no means in the position it might be.

One had only to instance the mechanical precision, over great distances, of our large pieces of ordnance, to bring the fact out that we had nothing to compare with it in the skiagraphy of the localisation of foreign bodies.

The basis of all "localisation" was the relation of the sides of various triangles or parallelograms formed of straight lines, some of which existed objectively in space between equally objective points. One spoke of the "central" and "vertical" ray as if it were an easily determined and subjective factor; he had begun to doubt the accuracy of the usual methods of so-called "finding" of these factors of the Tube. If a point was to be made of a Ray, as being a straight line, it was equally essential that the lines of surface in relation with it, horizontally or vertically, be also known. There was no doubt that many plates were exposed without any mechanical relation in this respect whatever. With regard to accuracy in this matter, the ideal seemed to be that the Tube and Plate should be "caged" and "anchored" together, whatever mechanical traverse be permitted between the two; but this would entail some drastic alterations in our present apparatus, with much more accurate workmanship in their making than at present obtains.

This apparatus is a sliding Cassette, its upper surface is very carefully cross-wired, its under-surface similarly grooved to take the sliding gauges, parallel to the surface and at right angles to the edge of the plate. The gauges are adaptable from any edge of the contained plate, and consist of a central marker of aluminium, engraved to take ink or dye, to mark the part immediately above it, this centres from any side, and is capable of being screened from below, also of a vertical gauge, the column and slide of which is marked in millimetres, and carries the "pointer," which can be set, to centre the cross-wire, at any point above it, within the limit of the column.

The subject with the foreign body, and the cassette, is now arranged. The aluminium marker, inked, is now engaged and centered, the part is pressed down, the marker withdrawn. The vertical gauge is now adapted, set to centre, and the vertical point above the part centred marked, the reading of the gauge is taken, the "pointer" gauge is now readjusted, as far as can be known about the plane of the foreign body, preferably in relation to some anatomical point, especially bone, the readings of the gauge are again noted. Two exposures are now made on either side of the vertical, sufficient to make a distinct shift of image; undetermined as far as traverse and distance of Tube is concerned, the plate is developed, one at

once sees the difference of shift in relation to the marker, you now measure the shift in millimetres of both the foreign body and the "marker" or "pointer"; and proceed so,

$$\frac{\text{Height of gauge m/m} \times \text{Shift of F.B. m/m}}{\text{Shift of Gauge m/m}} =$$

Height of F.B. m/m,

which shows that should the shift of the F.B. and Gauge be the same the height of the gauge becomes the height of the F.B. from the plate.

The speaker made a point of the centering of the foreign body on the centre of the plate, and using, as far as possible, a vertical ray; any oblique rays used to obtain shift pictures near the edge of the plate were inaccurate.

He also pointed out that all the errors connected with the tube came on both sides of the equation, and that altogether the device was simply an attempt to produce a gauge in definite relation to partially "anchored" plate, to co-relate certain factors and eliminate others, where the greatest errors crept in. In localisation of foreign bodies if these disturbing factors could be dealt with, while at the same time, accuracy and quickness could be obtained, such needed, nowadays, no urging on his part. The Casette permitted without disturbing the part, of taking stereoscopic plates of the centred detail with or without the marker.

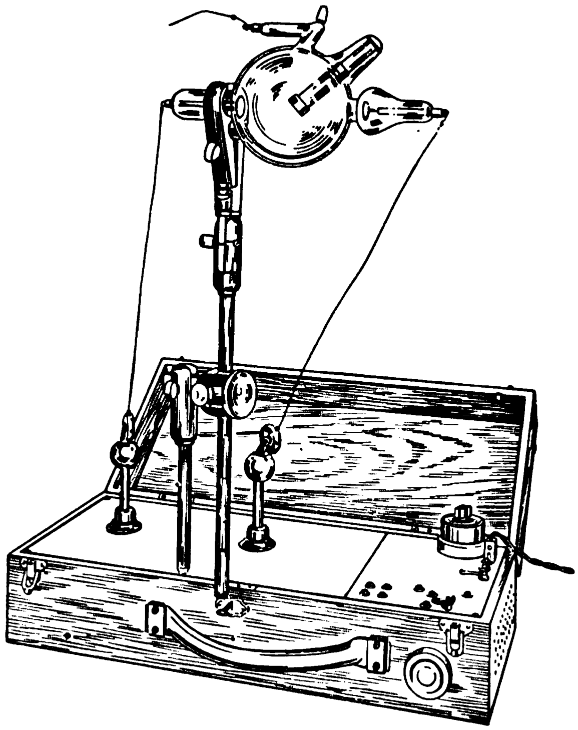
A vote of thanks was accorded to Dr. Harvey on the motion of the President.

THE GENERAL ELECTRIC COMPANY, LTD., LONDON.

THE DUBILIER X-RAY APPARATUS.

Exhibited by Mr. W. KRAUSE.

This apparatus is designed to work on any ordinary domestic supply of electric current and produces X-rays in a manner different to that usually adopted. It consists of a small electro-magnetic coil of about 30/40 ohms resistance and taking approximately 3 ampères. The current is taken direct from any convenient lampholder or socket through a vibratory spring contact. Each time the contact is broken an induced current of high potential is set up in the coil, the discharge taking place into a suitable condenser, the "outer coatings" of which are in series with the primary windings of a Tesla coil and again in series with another condenser with "outer coatings" connected to the opposite pole of the supply mains, thereby setting up a series of waves of high frequency



which are again transformed to a higher potential in the secondary windings of the Tesla coil. The current surges from condenser "A" to condenser "B" through the primary of the Tesla coil, and is therefore bi-polar in its action, but is almost wholly rectified by a tube of suitable construction.

The smaller of the two apparatus shown working with a 5-in. tube is of great advantage in Dental practice, and, as also the larger one, operates on either continuous or alternating current, is easily portable and convenient, weighing only 28 lbs.

RÖNTGEN SOCIETY.

A GENERAL MEETING of the Society was held at the Institution of Electrical Engineers on Tuesday, February 1st, 1916, Mr. J. H. Gardiner, F.C.S., in the Chair.

The Minutes of the previous meeting were read and confirmed, and the following were unanimously elected members of the Society by show of hands :—

Dr. R. LEDOUX LEBARD.
C. H. BROOKS, M.D.
JOSEPH E. BARNARD.
Miss ALICE MAUD ASHWIN.
T. CLARK.
CHARLES EDWARD LAWRENCE.

Nominations approved by the Council, for ballot at the next meeting.

JOHN FREDERICK REY, 9, Park Street, Bognor.
Proposed by R. Knox.
Seconded by Charles A. Clarke.

JOSEPH WILLIAM MASON, 1, Crouch Road, Stone Bridge Park, N.W.
Proposed by G. Pearce.
Seconded by R. Knox.

Dr. W. H. SYME, 63, Worcester Road, Christchurch, New Zealand.
Proposed by R. Knox.
Seconded by Sidney Russ.

Dr. Herschell Harris, of Sydney, who had just arrived in England after service at Gallipoli, exhibited an interesting series of lantern slides, and gave some particulars of the recent evacuation and the work of the R.A.M.C. In the course of his remarks he made generous reference to the respect observed by the Turks for our hospital ships, and referred to them as clean fighters.

The official business of the evening opened with a discussion on "The Injurious Effects produced by X-Rays." As announced in the agenda, the subject was introduced by SIDNEY RUSS, D.Sc.

THE INJURIOUS EFFECTS PRODUCED BY X-RAYS.

Dr. SIDNEY RUSS in opening the discussion on this subject said: I propose to take up five minutes of your time in explaining why I think that the injuries produced by X-rays form a good and suitable subject for discussion at this Society. I think it is desirable that we should discuss this matter in view of the various opinions that are held on the subject. In the first place there are plenty of people who have never seen any injurious effects from X-rays at all. There are other people who know of these injurious effects and have seen them. And there are still other people who suffer from them. This being so, it is only natural that there should be a good many different opinions and various shades of opinion as to the relative importance of these dangers and of the most appropriate means by which they can be countered. Although the subject has a strictly medical side, it has many other aspects in addition, and on the question as to its suitability for discussion before this Society I should like to say a few words.

It may surprise some of those present to know that more than one-half of the members of this Society—53 per cent.—are medical men. There is a natural diffidence on the part of medical men to discuss strictly medical matters outside a medical society, and that is where the Röntgen Society is fortunately placed, because it is so balanced that the medical men can say just as much and just as little on medical matters as they choose, and it is, of course, the only opportunity, practically speaking, that non-medical members have of getting the information which medical men can give them.

The second point on which I wish to dwell is as to the nature of the X-ray dangers. We may say that they are twofold ; in the first place, there is the danger through their misapplication, and in the second place, the danger to the man who is operating. It is this latter aspect which, I think, is most suitable for discussion on this occasion. These dangers fall into two broad classes. There are obvious

dangers, and there are hidden dangers. It is not my purpose to show you pictures of the obvious dangers of X-rays—that is to say, the superficial ones ; some of the pioneers of X-ray work have suffered very severely from superficial lesions which have not always remained superficial, and to bring these before you might give you a rather unbalanced idea of X-ray dangers—unbalanced, I mean, because they are perhaps not such immediate dangers as those which now confront the men who have to deal with X-rays.

There is another group of dangers, however, of which I should like to speak at greater length. These are the hidden dangers. Such dangers are only manifested in the course of time as the years go by. They are cumulative, and are, I think, worthy of the very strictest attention. I would mention particularly among other physiological effects the blood changes which occur under exposure to X-rays. From the paper of M. Aubertin on the gradual changes set up in the blood by X-ray exposures which were sufficient to produce no obvious lesion, I have taken some statistics which I place before you in the form of a lantern slide. The first column gives the age of the people examined, and the second the period over which X-rays were used. These people suffered from no obvious lesions whatever. The observations were made on the content of the red and white blood-cells, the figures for the normal man being taken as 5,000,000 red cells and 10,000 white cells. In every case the red-cell content is shown to be below the normal, and in most of the cases the white-cell content is also below the normal. The most striking thing is that, when the count is made of the different varieties of white cells, the diminution is so much more marked in certain of the white cells than in others. In fact, some of them showed a marked increase, and others a marked decrease. The lymphocytes in almost every case were well below the normal. This is not by any means an isolated instance, and a series of systematic observations have been made by Mr. Purvis in the United States, but the final report is not yet available.

Now the question arises as to whether the dangers of X-rays are becoming more or less formidable, and I think we have to recognise that they are becoming less formidable on account of the wider appreciation of the dangers which exist. But we ought also to remember, on the other hand, that the greater output of radiation now available makes it necessary to take still more stringent precautions. The output of radiation from the modern X-ray apparatus is now just about fifty times what it was ten years ago, with the apparatus then available. Are our precautionary measures running parallel with this enormous increase in output ? Then there are more dangers on account of the greater amount of radiological work which is being done. Perhaps it is not a safe guide to deal with percentages, and we must look on the total amount of damage done as an index of the dangers existing. I thought it worth while to look into the literature to see whether it might serve as an index as to whether these dangers were becoming more or less formidable.

My next slide will show you what the literature on the subject has to tell us. I have consulted the literature for the last twelve years—that is to say, I have looked up the references to papers dealing with accidents arising from X-rays. (Dr. Russ here showed the number of such papers for each year worked out in the form of a curve, fluctuating year by year, but reaching a very high point in 1909). Out of that literature the classification of accidents is often coupled with direct warnings. The writers will often go out of their way to express warnings to their confrères as to the dangers confronting them. The only purpose I have in bringing this before you is to submit that the dangers are still amongst us. It is not meant in any way as a quantitative study. If we find that in 1909 there were thirty papers dealing with accidents, and in 1910 only ten papers, it does not mean that there were three times as many accidents in the one period as there were in the other. One of the papers I consulted, for instance, dealt with no fewer than forty-seven cases of X-ray ulcers. But from the warnings which are issued in the literature of the subject and from the number of cases which are chronicled there is unmistakable evidence that the dangers are

still existent. I have stopped short in 1914, because I need not remind you of the enormous increase in X-ray work which the present war is entailing and of the special risks involved therein.

The next and last consideration is as to how these dangers are to be met. It seems to me that we must have some constructive method by which the thing can be attacked. I may remind you that the work of the Society during the last year or two has furthered it in some respects. You are aware that the Röntgen Society has drawn up rules of procedure, which, if they are followed, will do a great deal to obviate the dangers, and the time may come when certificates of safety may be looked for. But perhaps that is not going as far as the Society might go. The subject appears to me such a big one that one has to look round in many directions in order to discover how the dangers can best be obviated, and the question of legislation is no new one. The question is whether X-ray procedure requires to be done under legislation. I have not the Blue Book to quote from, and I do not know whether more deaths have resulted from phossy jaw or lead poisoning than from X-rays. The question for us, however, is not what other subjects now come under legislation, but whether the whole thing could be met by legislation more satisfactorily than by other methods we might evolve. Legislation would be the least popular method, and the least convenient. There is at present no legislation whatever with regard to the use and practice of X-rays. There is nothing in this country to stop anyone from using X-rays under any conditions that he likes ; and it seems to me a very great opportunity for the Röntgen Society to try and see what constructive methods it could put into practice whereby we might say definitely that the dangers involved in the use of X-rays do not so far as this country is concerned exist any longer.

Dr. REGINALD MORTON : Dr. Russ has spoken of obvious and of hidden dangers. As regards the obvious dangers, these have been with us for many years, and are very well countered on the whole by modern methods of protection. On reading over the literature of the subject, I am rather inclined to think that at times there is a tendency to attach too much importance to the dangers of X-rays. I take the view that as long as one exercises certain common-sense rules, using ordinary protective devices, and taking care to keep out of the way of the beam of rays as much as possible, the risks of injury are rather small. I speak as one who has been dabbling in X-rays ever since their discovery in 1895, and has so far suffered no inconvenience. With regard to the hidden dangers, our knowledge is very incomplete. I notice Dr. Russ made no particular mention of the question of secondary rays. At the meeting of this Society at the Cancer Hospital in June of last year, we heard a good deal about secondary radiations, and some speakers dilated on that danger fairly strongly on that occasion. Personally I am not inclined to attach a great deal of importance to secondary radiation. If secondary radiation is a danger, it is almost impossible for the radiologist to keep out of it. Moreover, there have been various attempts to make practical use of the secondary radiations. Dr. Hernaman-Johnson, in particular, has devoted himself to this subject, but I imagine from his silence that he has reached nothing very conclusive. Whatever the physicist may have to show with regard to them, from the physiological point of view secondary radiations have very little effect. There is another question raised by Dr. Russ this evening, namely, the cumulative action of the rays. That is also mentioned on the set of rules which the Society has provided. If the effects of X-rays are steadily and consistently cumulative, workers like Sir James Mackenzie Davidson and myself would have withered away long since. The fact is, we are capable of receiving a certain dose of X-rays and making a complete recovery therefrom without suffering any permanent disability. That gives us a margin of safety. I have had countless thousands of these X-ray doses, but I have not suffered any inconvenience ; and that means that we must be in a position to throw off the effects of a certain amount of

X-rays. What that dose is, approximately, I am not prepared to say. Mr. Donnithorne has shown us a very elaborate X-ray couch. This is interesting, but at the same time it has to be remembered that the rays which are the most dangerous are the easiest to deal with. There is one point with regard to the possibly harmful effects of X-rays on which I can speak from personal experience. The number of patients to be examined in an afternoon at the hospital fluctuates considerably, but some days it runs up to as many as thirty. At the end of such an afternoon, it is a common experience for me to feel so dead tired that I could almost drop asleep. Is this feeling due to the direct effect of small doses of X-rays, or it is the effect of the ionization of the air? I myself would prefer to think it the result of the small doses, because there one has something tangible to get hold of. The ventilation of the rooms will not help one much. I have felt the same thing in X-ray departments where there has been installed the most perfect system of ventilation; and changing the air of the rooms does not get rid of the ionization. I wish to say, in conclusion, that my remarks are not intended to in any way minimize the dangers of the X-rays; they are far too real for that; but there is nothing to be gained by exaggerating them, and it is just as well occasionally to look upon the brighter side.

Dr. W. HARWOOD NUTT: I have in my hand two precautionary notices, one of which I have received from the Government, and the other from the Röntgen Society. Both of them concern the precautions to be observed by X-ray operators. When I received these two notices, I could not help feeling amused, because I know that with many of the installations provided, it is simply impossible to carry out the rules. It is a physical impossibility to fulfil the obligations laid down. I am more convinced on this matter as a result of the discussion this evening, in spite of Dr. Morton's optimism. Operators who have been at work in France have returned to this country after three or four months with very bad ulcers and dermatitis on their arms and hands.

I have made some photographic measurements of the rays emitted from an X-ray box. My own opinion is that it is dangerous anywhere in an X-ray room with a tube in operation, unless the tube box is efficiently protected, no matter whether one stands in front, of or behind the tube.

I do not believe that any man can protect himself adequately unless the tube itself is sufficiently guarded.

I know of one installation where the operator has sheathed his arms and wrists in tubes of sheet lead and worn a lead-lined helmet; at the same time the fixed aperture of the tube box with which he was actually working day by day was 4″ or 5″ square. He was really living in a bath of X-rays. I cannot conceive anything more dangerous in spite of his lead armament.

I do agree that there ought to be a closer and more hearty co-operation between the medical men and operator, the physicist and the manufacturer. Unless these work in co-operation, I do not see how the rules brought forward are going to be carried out.

I feel quite sure that I am right in saying that not 5 per cent. of the apparatus at present in use is thoroughly safe.

I am illustrating on the screen a case of X-ray burn on the left hand and wrist, showing the rapid progress of the ulceration. The patient was under the care of Captain Stevens at the Wharncliffe Hospital. He was a lay operator and had been working with X-rays for only three months in France. This man came in with a certain amount of dermatitis, with several small ulcers, a red and glossy skin and some pigmentation. Curiously enough he did not suffer much pain. He was with us for only two weeks or so, and was then discharged to another hospital. In that time the ulcers rapidly increased in size, in spite of treatment. Either the apparatus was inadequately protected—it was known as a field outfit—or the operator was ignorant of the risks he ran.

X-Ray Burn on Hand and Wrist.

Dermalitis with several small ulcers broken out since admission.

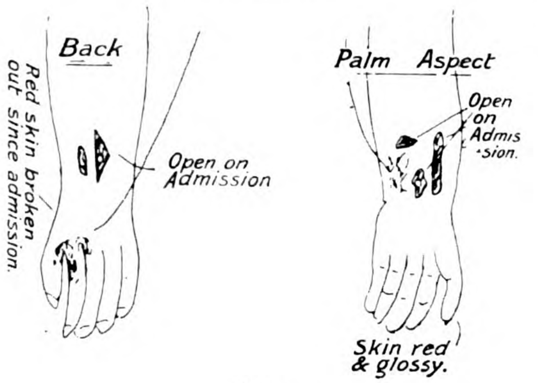


Fig. 1.

My own opinion was that both were at fault. I myself received an installation from the Government, the box of which was lead-rubber lined. On exciting the tube and using a screen, I found that a great deal of radiation came out in every direction.

Obviously the first thing to do was to find the weakness of the outfit and to rectify it. To do this I disposed a number of X-ray plates about the room at convenient points and then screened and radiographed fifteen average cases with the following results :—

Plan of X-Ray Room and Couch.

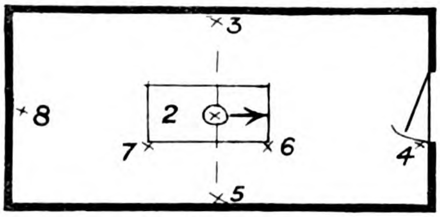


Fig. 1a.

Fig. 1A. The following table and slide shows the disposition of the plates about the room and couch, and their distances from the tube.

The amount of fogging on the plates is shown in the next slide (Fig. 2).

The density of the plates corresponds to the numbers under that head in Table I., and were estimated by the Sanger-Shepherd densimeter, and for purposes of comparison they are corrected for a uniform distance of one foot by multiplying the density by the square of the distance, giving the figures in the last column of the table.

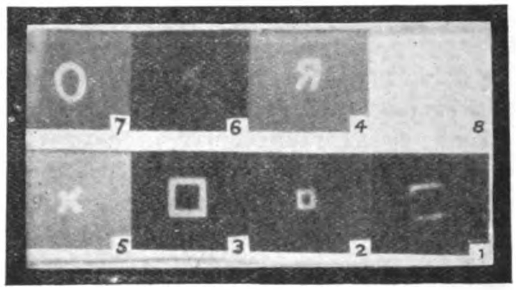


Fig. 2.

TABLE I.

Plate No.	Position of Plate.	Distance from A.K.	Density of Plate.	Relative Intensity.
1	Position of X-ray Operator	15″	14½	22
2	Top of Couch behind A.K.	1′	8	8
3	Left of Couch	6′	8¼	297
4	Facing Anti-kathode...	15′	4½	1012
5	Right of Couch	5′	3½	94
6	Right fore leg of Couch	3′	13½	121
7	Right back leg of Couch	3′	5½	50
8	Behind the Anti-kathode	12′	2	288

Fig. 3. The relative intensities can be shown graphically on the following slide.

Relative Intensities of Radiation around Couch.

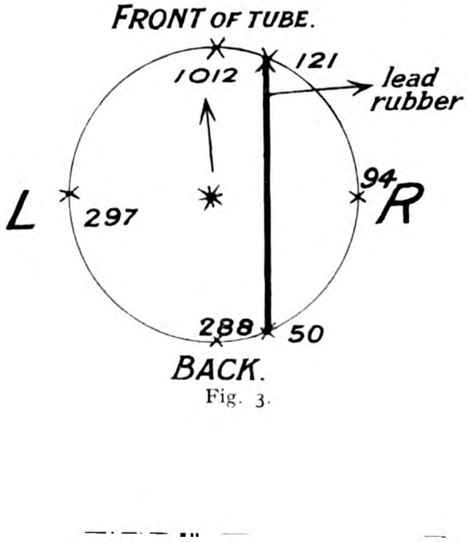


Fig. 3.

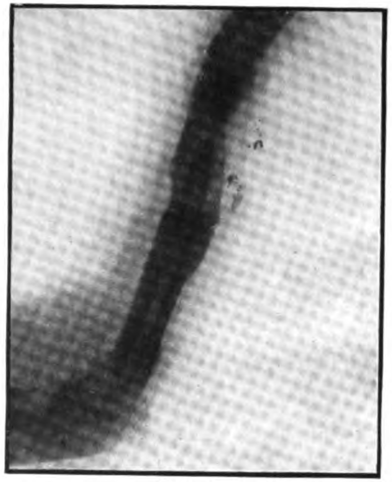


Fig. 1.


Fig. 2.

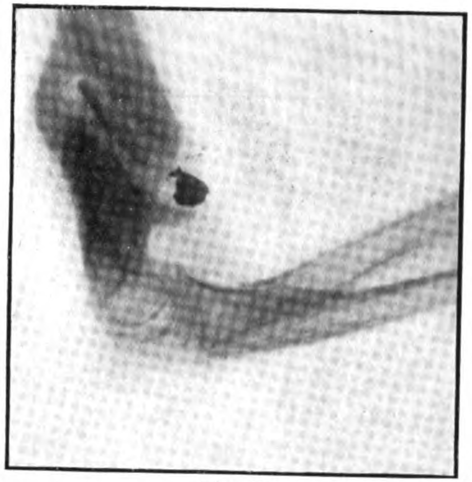


Fig. 3.


Fig. 4.

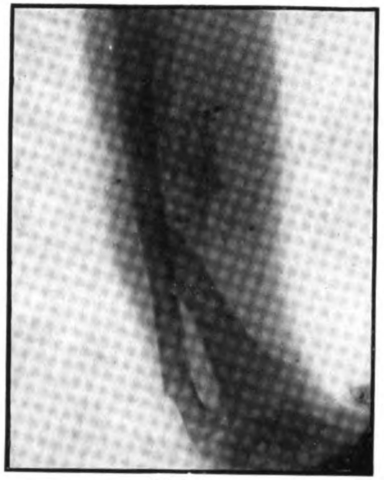


Fig. 5.

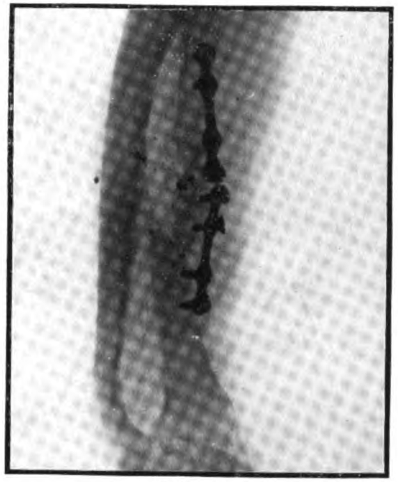


Fig. 6.

Examples of Bullet Injuries.—By C. A. SCHUNCK.

PLATE VI.

NATURAL DEPOSITS OF PITCHBLENDE *(The radio-active portions are shown in black).*

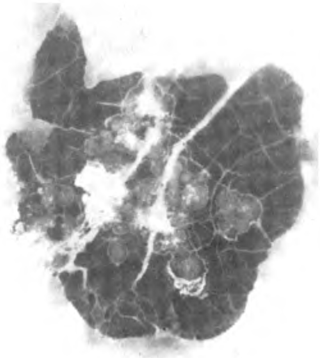


Fig. 1.—Section showing natural markings. St. Joachimsthal.

Fig. 2.—Section with fragments of metallic sulphides. Trenwith Mine, St. Ives, Cornwall.

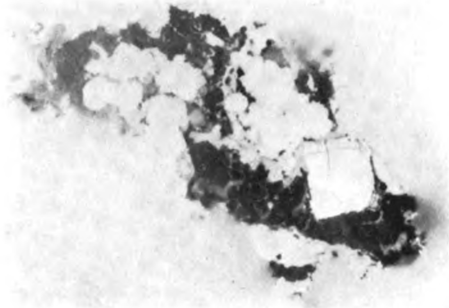


Fig. 3.—Section with crystal of Iron Pyrites. Trenwith Mine, St. Ives, Cornwall.

Fig. 4.—Section showing active matter in layers. Trenwith Mine, St. Ives, Cornwall.

Fig. 5.—Section showing active matter in zigzag formation. Trenwith Mine, St. Ives, Cornwall.

Fig. 6—Section showing crumpled formation of active matter. Trenwith Mine, St. Ives, Cornwall.

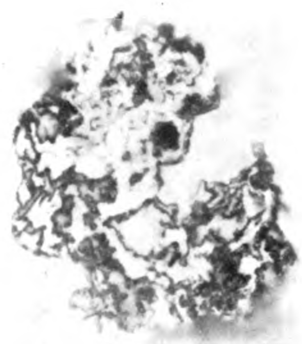


Fig. 7.—Same as last, by β and δ rays only.

The Occurrence of Uraninite (Pitchblende).—By J. H. GARDINER.

PLATE IV.

NATURAL DEPOSITS OF PITCHBLENDE *(The radio-active portions are shown in black)*

Fig. 8.—"LIGHT PHOTOGRAPH," Pitchblende and Pyrites.
Trenwith Mine, St. Ives, Cornwall.

Fig. 9.—Radiograph of last specimen.

Fig. 10.—"LIGHT PHOTOGRAPH," by reflection at an angle.

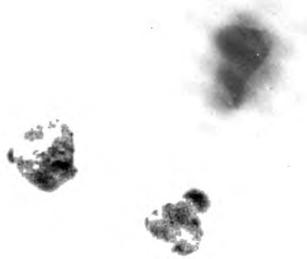


Fig. 11.—Crystals of Broggerite ("Anneröd," Norway).

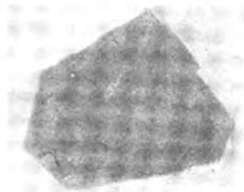


Fig. 12.—Crystal of Broggerite ("Anneröd," Norway).

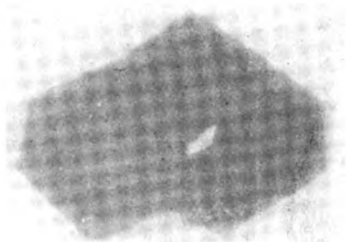


Fig. 13.—Twin Crystal of Broggerite ("Anneröd," Norway).

The Occurrence of Uraninite.—By J. H. GARDINER.

PLATE V.

They can be mapped out also in a single curve. (Fig. 4).

DISTRIBUTION OF RAYS
AROUND TUBE BOX.

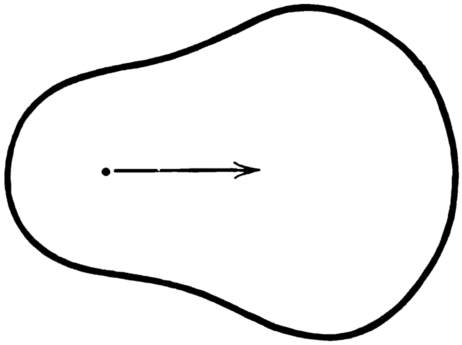


Fig. 4.

There are several striking points brought out by these results.

(1) When standing behind the tube you receive nearly four times less of the rays than when in front with the same protection.

(2) By covering the right-hand side and the top of the box with an extra sheet of lead rubber, 1·12 m/m. in thickness, you cut out two-thirds of the escaping rays, as is shown by comparing Nos. 3 and 5, whose relative densities are 297 and 94 respectively.

(3) No. 1 is of special interest, as it was exposed in the position of the operator, and it illustrates the relative amount of rays received by him, that is 22 in the table, during the time taken to give ten average exposures, whereas the other plates were exposed for fifteen average cases.

We must note also that there was an extra sheet of lead-rubber 1·12 m/m. in thickness interposed between the box and the plate.

(4) Again, by taking the relative densities of those positions which were protected by the extra sheet of lead rubber, Nos. 5, 6, 7, in comparison with those unprotected positions, Nos. 3, 4, 8, we get the following results :—

The mean intensity of the former set is 88, whereas that of the unprotected set is 532, that is about six times as great. It shows clearly that two thicknesses of lead rubber measuring 1·22 m/m. and 1·12 m/m. respectively are not an efficient protection against the rays.

These figures are purely relative and do not denote any recognised amount or measurement of rays, but they do force upon our attention the enormous amount of rays received by an operator examining, say, on an average, fifteen cases daily throughout the year. It is this constant daily fractional dosage which is calculated to do some definite damage, although it is not immediately obvious.

To get over these difficulties of exposure to escaping rays, there are three measures to be considered. They are briefly as follows :—

(1) The protective measures on the box itself.
(2) The protective measures on the couch.
(3) The protective measures adopted by the operator on his own person.

The first of these is the most important of all ; the second and third are supplementary. The only effective way to secure safety is to use lead sheeting to line the top and the operator's side of the tube box, in addition to an adjustable rectangular diaphragm and the internal lead-rubber lining of the box. The second consideration is important in view of the wide distribution of secondary and scattered rays when the diaphragm is opened. Briefly, the sensible thing to do is to fix a sheet lead apron to an upright connected with the carriage of the tube box, and sliding on a rail running the length of the couch. It must then move with the tube and always be a protection to the operator's body. To protect assistants, nothing can be better than to cover the whole of the far side of the couch with sheet lead.

To catch the widely-scattered and secondary rays emitted from the top of the couch, I find nothing better than having a loose sheet of lead on the couch top, which is moved about in front of the operator, always just missing the part of the patient being X-rayed.

If the above means are taken, it appears to me that screening becomes a safe procedure, providing that the diaphragm is always kept cut down to the smallest limits, and good gloves are

worn for the protection of the hands and arms, the parts most affected by burns and dermatitis. It also becomes unnecessary foı the operator to take refuge in lead-lined cabinets, fıom which he cannot possibly do his work of searching for foreign bodies. Also the X-ray plate was never designed to take the place of the hand-screen.

I should like to make one or two comments on the various points raised by the speakers in discussion. I think that the Society should pass some resolution at this meeting intimating to the Government that inspection is required for every X-ray installation in the country which is under the authority of the Government itself. I am not speaking of private installations. The Society ought to take some measures to induce the Government to give them a certificate of safety, and if the Government appoint inspectors, they ought to appoint medical men, and not physicists, and not laymen. Dr. Morton has mentioned the feeling of inertia and of being "done up" after a few hours in the X-ray room, and has suggested that this might be due to the ionization of the air. I have had in my own X-ray room two electric fans, one drawing air into the room, and the other drawing it out, so that a constant current was passing through. This made the room tolerable. I never go out feeling tired or "done up," even after X-raying thirty or forty cases without intermission—a proof that ventilation does improve matters. One ventilator, however, is not sufficient; one must have an inlet and an outlet, each worked by an electric fan.

Dr. N. S. FINZI: The question of danger to the operator resolves itself into two main branches, namely, danger to the operator when treating patients with X-rays, and danger to the operator when examining patients with X-rays. They are rather different subjects. When treating a patient with X-rays we may imagine the patient 9-in. from the tube—in ordinary treatment that is a very common distance—and the operator 3 yards from the tube. If the operator were exposed to the full stream of X-ıays which falls upon the patient, the fact that he is twelve times as far from the tube as the patient is makes it evident that the operator is getting 144th part of the patient's dose. Therefore, without any protection, the operator would have to X-ray 144 patients in order to receive himself the full force of the X-rays which strike one patient. Suppose, however, that only feeble protection is afforded, and that the protective shield cuts off only 80 per cent. of the rays; then the operator would have to X-ray over 700 patients before he himself received one pastille dose. Less than a pastille dose, especially if spread over a long period, is not going to do anyone any harm. Therefore, if the operator adopts the precaution of merely standing three yards from his apparatus, he is going to be pretty safe in any X-ray treatment.

We come then to the question of diagnosis. In this case, to do efficient work, a man has to stand close to his apparatus. He cannot do efficient work without screening, and he cannot screen without bringing himself into close proximity to the tube. Mr. Donnithorne's figures, given on page 47, are extremely interesting as being taken from a couch exceptionally well protected, and they show that with a diaphragm widely open one still gets a proportion of rays. He has shown that closing the diaphragm makes a very great difference to the amount of radiation that strikes the operator, and it also makes a very great difference to the resulting picture, so that it is important always to use as small a diaphragm as possible. There is another reason for always using a small diaphragm; unless the area open is completely covered by the lead glass of the fluorescent screen, rays will be escaping into the face of the operator and his assistants. Mr. Donnithorne's figures show the advisability of the operator wearing the protective apron and gloves. One of my assistants, Captain Stone, tested the amount of rays striking him by putting little enclosed pastilles on the front and on the back of his protective apron. On the front of his apron the pastille turned to one-fourth of the "B" tint in three months. During this time he had been engaged closely on X-ray work (every morning, and four

afternoons a week). But the pastille on the back of the apron was still unturned in three months. The amount received by the operator is four times that recorded by the pastille. Therefore if the worker is behind his protective apron, even if he is doing a considerable amount of work, a pastille dose in three to six months does not seem an amount likely to do him any harm, even if continued over a considerable period.

The other great point in examining patients is to keep the hands out of the rays. The wearing of gloves is a little bit of a snare (although it should always be done), because the operator tends to think himself fully protected when the gloves are being worn, but no glove protects completely, especially if old and cracked. He should keep his hands out of the way, and use screen rests or sand-bags and blocks ; he should not hold the screen under any circumstances, except when absolutely necessary to move it during the examination. The necessity of ventilating an X-ray room is obvious. We have almost all of us the tired feeling after a hard day's work. I have always thought it was due to the sheer physical and mental exertion of examining a number of cases.

The danger to the patient when the treatment is carried out by competent men is negligible. We know exactly the doses required to produce certain effects, and an accident is generally the fault of the radiologist. The danger from late X-ray effects is a very different thing. Such cases are extremely rare, but we do not know how to prevent them, we do not know their cause, and we do not know how much radiation is necessary to produce them. In using X-rays for diagnosis, burns are also the fault of the operator. Some couches have been constructed which rather increase this danger. I have already emphasized the importance of maintaining a sufficient distance from the source of the rays. Recently I have seen a couch in which the furthest distance which the anti-cathode can be placed from the top of the table is 13 in., and the nearest distance is 7 in. This latter would be very dangerous in the hands of the inexpert operator. In any bismuth case, where the whole intestinal tract of the patient has to be radiographed several times, it is essential that the anti-cathode shall not be nearer the nearest point on the patient than 15 in., better 18 in.

Dr. F. BAILEY : (Brighton) : I do not know that I can speak at any length with regard to this subject. When one is engaged upon it more or less from the medical point of view, possibly one's notions regarding the scientific points involved are scarcely worth bringing before a Society such as this. There is, however, one point in connection with practical work upon which great stress should be laid. From the earliest days of X-rays, the tendency has been to supply the X-ray worker with one of the darkest and smallest rooms in the hospital building. We all know that the danger from X-rays is very largely proportional to the proximity of the worker to the tube, and this is one point, I think, that should be impressed on all the authorities who are engaged in the arranging of X-ray departments. One must not say too much about matters connected with the Army, but here again I do not think this point has been sufficiently taken into consideration. We have large wards and tiny X-ray rooms, and it is a matter of great difficulty for the workers when the tubes are in operation to get away a sufficient distance from the apparatus. I myself have been working with X-rays for a considerable number of years, and I am one of those fortunate individuals who, either through some idiosyncracy, or through taking care, have not suffered from any of the superficial skin lesions, so far as I know. During that time, a number of patients have been under me for treatment of malignant disease, and I have come to the conclusion that many of these patients—some of whom, perhaps, have been under treatment for four years—while remaining in excellent bodily health, show a tendency gradually to develop a certain amount of anæmia. This seems to agree with what Dr. Russ has pointed out to us. Some time ago I more or less re-organized the whole of my apparatus (which had got spread out and cumbrous),

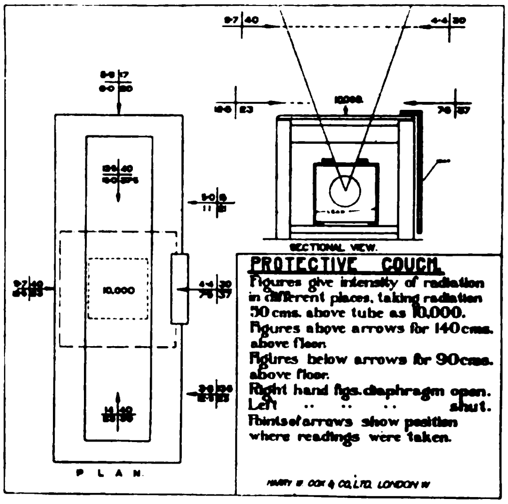
and in doing so I went over the new apparatus carefully—it was made by one of the first makers in London—to see what amount of protection it was really affording. I was surprised to find that, practically, when any quantity of milli-ampèrage was going through the tube, the protection was nothing worth speaking of. The X-rays crept out at many points, including the back of the tube, and there was, apparently, secondary radiation from the iron in the apparatus. One may hope that makers will give more consideration to this matter, and will test their apparatus as it goes out with the fluorescent screen, in order to see whether there is any considerable amount of X-rays coming out in an unwanted direction. Personally, I am rather careful; when I am working I employ a large screen, about as big as myself, the upper third of it being of lead glass, and the whole running on castors. I take it about with me, and hide behind it. I really do not think one can be too careful, but in treating a patient my tube is enclosed in a large lead-glass bulb, and with this and the big lead screen it is scarcely likely that any direct X-rays can reach the operator at all. The question remains as to whether the ionization of the air by X-rays may not be a dangerous thing for X-ray workers. To obviate the danger of secondary rays, I am not very much in favour of the metallic couches, and am rather inclined to stick to the old wooden type. I would have gone into the question of one's patients who receive such a tremendous amount of X-rays and yet, under conditions of filtering, show no dermatitis whatever, and do not even lose their hair; but that is a little apart from the question we are discussing at present. I would like to hear Dr. Russ's views on the question of air ionization.

Dr. G. H. RODMAN: I can speak from little experience as compared with many of the other workers present, but I do feel that the Society is fortunate in having heard such lucid statements as those propounded by Dr. Russ. Some years ago, in speaking before this Society, I voiced the opinion that this danger in the use of X-rays was a very material one, and hinted at one or two courses the Society might take. Dr. Russ has echoed this, especially in what he has said about the possibility of legislation. At the time I brought forward this question, our legislators were engaged on work which they considered far more important, and though I made it my business to approach my friend, Sir George Cave, the present Solicitor General, on the subject, I suppose that in the pressure of business the matter was allowed to slide. We might, as a Society, do a great deal with another body, to whom I then submitted my suggestions, and which seems to me to have a function which might be beneficially exercised in this direction. I refer to the General Medical Council. One very suitable way in which we could be helped would be by the General Medical Council taking up the question and possibly including some measure of X-ray practice in the medical curriculum. It seemed to me at that time, as now, that the General Medical Council looks after the interest of the general public very well indeed, in that they ensure that before a man is allowed his qualification he has to show that he is perfectly cognisant of the dangers of such a drug as strychnine. In the X-ray we have an agent of far more subtle danger than the small amount of strychnine that is prescribed. With regard to dermatitis, I contracted a dermatitis in the early days of my work on my left hand, and was very exercised about it, because it did not seem to get well. I took the opinions of one or two of my colleagues, and they were vastly different. One man urged me to treat it with radium, another to submit it to the action of a low temperature in the shape of carbonic acid snow. I thought that, as all this was to my mind purely experimental, I might by accepting either advice be jumping out of the frying pan into the fire, and possibly that by avoiding too frequent contact with the X-ray tube, and in addition to that, exposing the dermatitis as freely as possible to sunlight and fresh air, I might get a good result. Under that treatment my dermatitis has very much improved. During recent years I have been more frequently in proximity to the golf-course than to the X-ray tube, and by exposure to sunlight and fresh

air my dermatitis has practically cleared up. At the present time, with the exception of one nail on my left hand, where I have a fissure, I am practically well. I should like to ask whether anyone can suggest what it is that constitutes susceptibility to X-rays. Is it the complexion ? Is it a certain quality of skin ? Is it in any way allied to the condition that predisposes a man or woman to freckle or the reverse on exposure to sunlight ?

Mr. H. E. DONNITHORNE said that he could not speak with the experience of a medical man as to the harm which X-rays might do, but he would like to say something about the means of preventing X-ray injuries. In this connection he had worked out certain measurements which, with the permission of the meeting, he would show in the form of a lantern slide. The measurements concerned one of the latest models of the Cox couch, and after a view of the couch in its entirety, he explained that, fixed to the trolley on which the tube box was slung, was a vertical screen lined with lead, which moved along with the tube box and interposed itself between it and the operator. He had taken measurements to see what radiation would be received by the operator when standing in various positions in relation to the couch. In the diagram below thick lines were made to indicate the lead insulation. The measurements were taken at a height of 140 cm. above the floor, which was roughly the height of a man's face when stooping slightly, and also at 90 cm., which was about the height of a man's hands and of his delicate organs which were liable to receive injury.

The radiation passing through the top of the couch at a distance of 50 cm. from the anode was taken as 10,000, and the proportion received at the level of the face when the diaphragm was closed down was 4·4, and at the level of the hands 7·5. He noticed that the smaller the diaphragm opening, the more marked was the reduction of the scattered radiation. The reason why the figures increased so much when the diaphragm was open more widely was obviously due to secondary radiation coming from the walls of the tube, and this increased with the size of diaphragm opening. The first point, therefore, was that the worker should use



as small a diaphragm opening as possible. One of the most important points of all was to be gathered from the readings made in front of the couch where there was a second layer of protection. With the diaphragm closed down the radiation is represented by 4·4 at the face level of the operator ; and on the opposite side by 9·7. On opening the diaphragm, the figure at face level becomes 30 on the operator's side and without the shield 40, but at the level of the hands the shield is apparently doing harm, for there the intensity is 37, while on the other side it is only 23. When he laid the fluorescent screen upon the top of this shield, he generally got an image of the edge of the lead, but when he opened the diaphragm fully, the image of this hard line disappeared, and the screen got brightly illuminated. He believed that the secondary rays from the walls of the tube grazed the top of the lead at the corner. Professor Barkla, who had made a number of experiments on this subject of secondary radiation, had stated that the rays which grazed a metal surface gave rise to very strong secondary radiation—a secondary radiation far stronger than would be caused by a direct impingement. He (the speaker) found,

in the course of his own experiments, that if he went an inch or two from that part, the radiation decreased very rapidly.

When the diaphragm was closed, one got at the point specified an intensity of 7·5. The figures showed that the secondary radiation had a value of about 11 (*i.e.*, the average difference between the values with diaphragm shut and open). Therefore one might expect this secondary radiation of 11, plus the leakage of 7·5 to make an intensity of something like 18 or 20, instead of which it was found to be double. It seemed, therefore, as though the secondary radiation, represented by the value of 11, striking at this point the surface of the lead, gave rise to secondary radiation of a value of 18. If this 11 gave 18, what was the original 10,000 going to do if allowed to strike bare metal above the couch ? He drew the conclusion that with bare metal above the couch, it was very important to the operator to keep some distance away, as the soft secondary radiation in its immediate neighbourhood must be very intense. It might be interesting for those present to know that the figures produced showed that the lead protection was roughly equal in value to 1 in. thickness of the best lead rubber which Dr. Russ had spoken of at the June meeting of the Society.

Sir JAMES MACKENZIE DAVIDSON : I am sure the Society ought to feel very much indebted to Dr. Russ for opening this discussion in so admirable a manner. The prevention of X-Ray injuries is a matter of prime importance. We have had among our friends many victims of the more obvious changes which X-rays may bring about, and in addition to these there are deeper and hidden changes of whose extent we are at present unaware. The question has two aspects : the first, what is to be done with the victim after he has had the injuries inflicted upon him, and the second and more important, what is to be done to prevent the injury being inflicted ? This Society, it appears to me, is well fitted for the discussion of this question, because it is a meeting ground for both medical men and physicists. Knowledge is divided up into many phases, but it is well that it should not be contained in watertight compartments, and that occasionally the medical man should trench upon the preserves of the physicist, and the physicist in his turn upon those of the medical man. The object of this Society is to bring men together from opposite camps, as it were, in order that they may compare notes. The physicist in this instance can be of immense help to us. Many years ago this subject of X-ray protection engaged my own attention, because some of my friends had died as a result of X-ray injuries, and others were more or less maimed. In my early experiments I put my X-ray tube in a big box, burying it completely in red lead. Physicists had taught us that the heavy atoms were opaque to the passage of X-rays, and, fortunately, metals which themselves were good conductors provided us with oxides which were good insulators. I buried the tube so deeply that no X-rays could be detected outside the box. Then I scraped away the lead until the X-ray effect became visible, and I found that half an inch thickness of solid red lead powder was necessary in order completely to obstruct the rays. This, however, was not a practicable means of procedure, and I compromised by using a box lined with a putty made of white lead and red lead, completely enclosing the tube, save for a small aperture through which the beam of rays issued. But with the increase of screen work, it has become necessary to protect oneself in a special way when observing the screen, and therefore I always put a thick plate of lead glass in front of the screen. This has so far protected me, at least from the visible dangers, although the harm we may be doing to ourselves in the shape of more subtle ills is as yet unknown. Any efforts which the worker makes at screening himself should be very completely done. That is best achieved by enclosing the tube. I adopted the plan of using a thick lead screen such as is used for roofing purposes, and standing behind this, I was able to observe the images on the fluorescent screen by means of a mirror placed at an appropriate angle. By this means the worker can effectually protect himself. Not

much is lost if a good mirror is used. How far the atmospheric ionization or scattered radiation may cause injury to the X-ray worker I cannot say. The discussion this evening will help us a great deal in arriving at a more systematic protection of workers and nurses, and of all who use the X-ray tube, whether regularly or casually.

Dr. HERSCHELL HARRIS: I am interested more particularly in this question from the point of view of the appliances supplied to those on active service, and the small amount of protection afforded. I have had many opportunities of seeing installations in France and the Mediterranean, and in some cases it has been simply appalling to see what negligence has prevailed with regard to this matter of protection. The machines have been sent out without any supervision, and the suggestion made that some qualified person should inspect these appliances is an excellent one. One great fault is the glass supplied. Much of the glass is not lead glass, not X-ray proof at all. It seems to me an unnecessary risk to take, and the makers should be asked in all cases to notify their customers as to the quality of glass they are supplying. All the men working with X-rays are advised to take certain precautions, such as the wearing of long aprons. It seems to me well to stick to the old-fashioned long apron, and then one need not worry so much about the couch. I always wear a helmet. As to ventilation, no doubt the air is ionized, and a room properly ventilated is a much more pleasant place to work in. I think we should all work with as much ventilation as possible.

Dr. RUSS, in a preliminary reply, said: As the President has already indicated, it would be quite impossible for me to reply in detail at this juncture, and any more considered reply must be held over for the adjourned discussion. One point to which I may refer, however, is the question of ionized air, and the tired feeling which X-ray workers experience at the close of the day and which may be attributable to the condition of the atmosphere. I agree that the air is ionized, but I cannot see for my own part how any ions can get down into the lungs. Ions tend to disappear very quickly, and if the feeling of fatigue is due to ionized air, I would suggest that it could only be due to air in the lungs being ionized by the more penetrating X-rays. I would like to have a word from Professor Porter on that subject.

Professor PORTER said that he perfectly agreed with Dr. Russ. He could not make it more emphatic than that.

At this point the discussion was adjourned until the March meeting of the Society. The President called attention to the fact that in the library, where refreshments might be partaken, would be found a set of the new French journal, *Electrology and Radiology*, and the book by Drs. Colwell and Russ on *Radium X-rays and the Living Cell.*

DISCUSSION CONTINUED ON MARCH 7TH.

Mr. W. E. SCHALL: I think that one of the most striking points about the discussion at the last meeting was the great difference of opinion which seems to exist in regard to what is the correct protection from X-rays. And this is, no doubt, due to the fact that one operator looks to one particular type of ray, and one particular action of the rays, to be most injurious, and to be most guarded against, and another to another.

It seems to me, therefore, that before we can arrive at a somewhat unanimous conclusion about protection, we should endeavour to follow the X-ray on its path through the human body, and try to find out what process or series of processes it undergoes before the injurious effect becomes apparent to us.

When we consider the harmful effects of X-rays, we are faced with the problem of investigating a series of processes which are set in motion by the absorption in, or the passage through, the body, of the X-rays.

What, then, is the first of the processes? It may consist either in what may be called a trigger action of the X-rays, or in an actual absorption and transformation of energy.

In the first case the X-rays set up a physical or chemical process, in a way which may be compared to sending a bullet out of a rifle by pressing a trigger, or to the action of a catalytic agent in a chemical process. Once the process has been started, it goes on without further assistance, and the X-ray passes through and out of the body apparently undiminished in energy.

In the second case, however, the energy of the X-ray is absorbed by the tissue and transformed, and henceforward exists in some different shape, in a similar way to the absorption and transformation of radiant energy into motion by a radiometer, or into a chemical re-action, as in photographic plates.

We have then to decide which of these two initial processes takes place when X-rays pass into the body.

It is probable that we need not consider the trigger action at all. The effect produced when hard rays are used, that is to say rays which pass through, is much less than with soft rays which are absorbed, and it is conceivable that if we had a homogeneous bundle of rays, so hard that the dose entering, and the dose leaving the body are equal, no biological effect could be observed at all.

We come then, to the question of what kind of transformation of energy takes place when the X-rays are absorbed, and here we have to decide whether a physical, or a chemical process results, and what component of the body is most affected. It is here that the medical man and the biological chemist must take up the investigation, but as a layman in these matters, I want to draw attention to a review published in the "Archives" some time ago, of a book on the treatment of cancer by both X-rays and radium, in which the view was advanced that the gamma rays, when absorbed by the body, give rise to a radiation very similar to Beta rays, which in their turn set up electrical effects—chiefly ionization. These then accelerate chemical decompositions of the contents of the cells. Attention was furthermore drawn to the fact, that lecithin, which is present in cellular tissue when subjected to irradiation from X-rays or radium, changes to cholin, and an injection of the latter into the human body produces effects very similar to those set up by the X-rays. The conclusion is drawn that one way in which the rays act injuriously on the body is by electrically accelerating a chemical re-action, the result of which is a change of lecithin into cholin, the latter being the agent which is directly responsible for the harmful effects observed.

Be that as it may, the question of adequate protection which, at present, is the most important, depends almost wholly on the first point which I raised, namely whether the rays act as catalytic agents, or whether they must be wholly absorbed to give effect. If this meeting can arrive at a definite and unanimous conclusion on that point, we would, I think, have cleared the ground considerably. In the first case, it would obviously be necessary so to protect the operator, that not a single ray, no matter of what penetrating power, should reach him. According to Prof. Walter, some 2 m/m of lead, or 8 m/m of lead rubber, or 10-20 m/m of lead glass would be necessary, and no doubt the advent of the Coolidge Tube would tend to increase these figures. If, however, an absorption of energy only gives rise to X-ray effects, only those rays which cannot pass through the body are dangerous. In other words, we must determine what thickness of lead rubber, of density say 4·5, is equivalent in opacity to human tissue of the average thickness of the body, and we can rest assured that rays which are sufficiently hard to pass through this lead rubber will also pass through the body of the operator, and will, therefore, do no harm, and that all other rays will be stopped.

I want to put in a mild plea on behalf of the manufacturer. Practically at every debate which I have hitherto attended on the subject of X-ray protection, a certain amount of blame has been placed upon the manufacturer for not protecting his apparatus sufficiently.

Manufacturers are in a difficult position, and their difficulty is the lack of unanimity on the

question of protection, to which I referred at first—the uncertainty as to what we have to guard against. I do not suggest that there is not room for an improvement as regards protection—indeed the calling of this meeting is evidence of the necessity for improvement—and in some cases there may be more room than in others. But if we, the Röntgen Society, could make up our minds as to how much lead rubber must go round a tube to render it safe, what density this rubber must have, and how thick and dense our lead glass must be, the lot of the manufacturer would be a happier one, and he would be less likely to be blamed for being negligent.

Finally, I want to refer to two points that we raised at the last meeting. The first is, the protection of the couch shown by Mr. Donnithorne. I want to ask how the dose of X-rays was measured, and to say that I strongly suspect that it was by an electroscope, and, furthermore, that the knob of the instrument was not shut off from the air of the room. Thus the ionization of the air of the room was detected, and not the ionization in a small chamber due to direct radiation falling on it. My reason for the question is that the figures showing the dose of rays obtained laterally to the main stream of X-rays, and behind a thick sheet of lead, was much larger when the rectangular diaphragm on top of the tube box was open than when it was closed. This can hardly be in accordance with facts, and I think that the electroscope, or other measuring instrument, was merely recording the ionization effect due to a stream of X-rays being passed through the air.

The second point is, the question of the tired feeling due to working in the rays. I have experienced this feeling not only after testing tubes, or experimenting for any length of time with the rays, but also after having tested a coil through a spark gap for an hour or more. It has occurred to me that perhaps it is a question of ozone production.

W. HARWOOD NUTT: I regret that last month I had not the time to touch upon several important points, especially those suggested to me by the paper read at the June meeting by Dr. S. Russ on "The protective devices for X-ray operators." They will form the basis of my remarks to-night, and indicate the line of investigation.

Every one admits the importance of safe protective accessories. If the material is of an inferior quality, or does not come up to a supposed standard of excellence, they become a snare. The principal of these are the lead-glass screens and gloves. This table gives the result of an examination and clearly demonstrates the variation in the quality of the materials used in my installation.

TABLE II.

Material.	Thickness. m/ms.	Density.	Thickness for 1%.	% allowed through.
GLASS -	4·12 2·26	3·1	17	31 55
Rubber A	1·12	4·0	3·1	20
„ B	1·22	5·0	1·5	2⅓
„ C	2·5	4·0	3·1	4
Glove -	0·83	3·5	5·6	51⅓
Covering	1·32	4·5	1·8	3½
Glove & Covering	2·15	-	-	1¾

Table II.

In the first column the material is given, following that the thickness in millimetres, then the density and the thickness of the material required to cut out all but 1% of the rays. The percentages in the final column were obtained by assuming Russ's figures and curves for the relationship of the density of the lead glass or lead rubber to the thickness required to reduce the intensity of the radiation to 1%, and then applying the exponential absorption formula.

The sheet of lead glass first examined was accidentally broken. I found it varied in thickness from 2·26 m/m to 4·12 m/m. The density of the glass was 3·1, and required a thickness of 17 m/m to cut out all the rays with the exception of 1%. The thick part allowed 31% through, the thinnest part 55%. These figures clearly demonstrate the dangerous quality of the glass. In the resisting qualities of the glass itself there is a difference of 24%. Moreover the most resistive part of the screen, in my opinion, is much below what we ought to aim at.

The three samples of lead rubber form an interesting commentary on the quality of the material used for protective purposes on the tube box. The quality is far from uniform.

The one marked " B " is denser than either of the other two. It is half as thick as the third and very little thicker than the first, yet it allows through only half as much as the former and one-ninth of the latter. In addition, it is not inconveniently thick to cut out all but 1% of the radiation, which is an important fact when used in the manufacture of gloves and aprons where lightness is sought for.

In connection with the gloves used by X-ray workers, a very important question arises, that is, to get a serviceable glove which gives adequate protection to the most vulnerable part of the operator's body, and at the same time sufficient sensitiveness to work in comfort and to perform all necessary operations.

Most of the gloves supplied by the makers fall far short of these conditions. They sacrifice safety to pliability. As an example of this, I draw your attention to the details of Table II. The thickness of the gloves is 0·83 m/m, as you will observe a very thin material and of low density, 3·5. This glove allowed 51·3% of the incident rays to fall on the operator's hand. In addition, it is a glove made up in the usual manner with seams, which in time break down and expose part of the hand directly to the rays. To get over these two difficulties, and to give a real and not a sham protection, I have contrived a lead-rubber casing into which the gloved hand is introduced. This is seamless, and in it faults can be quickly detected. It gives almost perfect protection to the back of the hand, arm and fingers. The gloved thumb is exposed for the purpose of easy working. The particulars are as follows :—

Thickness	1·32 m/m
Density	4·5
Thickness to stop all but 1%	1·8 m/m
Percentage allowed through	3·5

The glove and the casing taken together have a combined thickness of 2·15 m/m, and allow through only 1·75%, a very satisfactory result, and much ahead of anything suggested hitherto.

It is obvious that these experiments fully bear out Russ's dictum " That it is density which furnishes a correct idea of the absorptive power of X-ray protective material," one which it is well to bear in mind.

I should also like to point out that it is not only material deficient in density which is responsible for many of the dangers to which X-ray workers are exposed, but that in some instances the requisites supplied are actually bad in quality. I have here a comparatively new apron from which the lead rubber is peeling, leaving nothing but the canvas backing. Some time ago I had a piece of " lead " glass sent to me for my fluorescent screen. In time I discovered that it was only a piece of thick soda glass with no trace of lead in it. I think these latter dangers which I have mentioned ought to be avoided. The former can be rectified with experience.

To determine to what extent the presence of ozone in the atmosphere of the X-ray room is responsible for the feeling of inertia and headache, I have had several experiments carried out.

The source of the ozone is undoubtedly to be found in the sparking which occurs whenever a high-tension current is excited, and especially when a hard tube is put into operation for deep radio-therapy. The strong odour immediately proclaims the fact that quantities of ozone are being generated. There are also the peroxides of hydrogen and nitrogen generated. Can the feeling of lassitude be ascribed to one of these alone, or to some extent to each of them ?

The experiments with a solution of potassium iodide and starch showed the qualitative test as a marked success, but the quantitative test was not so satisfactory. In the former the solution was placed in the tube box in an open dish. In five minutes it became a deep purple as a result of the ozone present turning out iodine from the iodide. The same experiment outside the tube box showed at the best the merest trace of ozone present. Again, 10 litres of the air of the room were collected during the usual course of the work with both ventilating fans stopped, and the windows closed. It was slowly aspirated through a triple absorption tube containing a twice normal solution of potassium iodide with the subsequent addition of starch solution, only a trace of colour was produced.

This was also the result of shaking up a four litre sample in a stoppered gasholder with the iodide and adding starch.

It is thus evident that the atmosphere of the room in which these experiments were carried out contained merely a trace of ozone. Since, however, the room was exceptionally large, it is still possible that in a more confined space the ozone which is certainly formed will attain a measurable concentration.

Whatever the gas responsible for X-ray tiredness may be, the only cure is a well-ventilated room by means of electric inlet and outlet fans.

Here I beg to acknowledge my indebtedness to Dr. Russ for the use I have made of his figures and curves, taken from his paper of June last, and also to Mr. F. C. Thompson, of M.Met., B.Sc., Sheffield University and Asst. Radiographer, Wharncliffe War Hospital, for the assistance he has given me in compiling and working out the results of the physical problems involved in this communication.

GEORGE B. BATTEN, M.D.: Until recently, I was strongly inclined to share the optimism of Dr. R. Morton, for after using X-rays for nineteen years I have never seen any visible injury to a worker's skin on any part of the body at all covered by clothing, and I have only been able to hear of one such case, that of poor Harry Cox, who, besides his face and hands, had his chest badly affected, and even in his case I am not sure that he did not do some of his earlier work with X-ray tubes with his shirt open.

More lately, however, I have become impressed by the hidden dangers which are probably present, and against which Dr. Russ has so wisely warned us, in the vastly increased output, both in quantity and in penetrative quality, of our modern tubes and outfits. Since the war began, the numbers of persons using X-rays have increased at least six-fold, and we all have used them more than previously. It behoves us, therefore, to be on the look out for fresh injurious effects beyond those we already know.

One of these risks I mention, as it may not be known to all, namely, the greater danger of dermatitis due to the combined action of X-rays and certain oxidizing and reducing agents, such as picric acid, pyrogallic acid, and other chemicals used in treatment and in photographic development. The only case of real dermatitis that I myself have seen, among all the cases that I have treated for ringworm and other diseases, was due to X-raying a head which had previously been treated by picric acid, and still had some of the yellow stain in the skin, when exposed to the X-rays.

I should like to point out that the pastilles mentioned by Dr. Finzi as changing to " tint B " behind his assistant's rubber apron, in a rather long period of time, is not so reassuring as he suggested—because these pastilles were at nearly the full distance from the tube, and not at the half-distance, and at the full distance they indicate a four-fold dose, not merely the ordinary, or epilation, dose.

Whether the hidden dangers are greater than the known ones to us veteran workers, is a matter of opinion, and mostly remains to be proved, and surely we are not a specially anæmic or sterile set of men, but I wish to emphasize the fact that in my opinion it is still the known dangers that are doing the greatest harm at the present time, when there is such a rush of work for the less experienced, and because the War Office, the instrument makers and these new workers do not pay sufficient

attention to the most excellent recommendations for the protection of X-ray operators issued by the Röntgen Society.

Major WILSON (a Canadian member) said: I do not know that I have any special communication to make, and I regret I was not able to be present at the first part of the discussion at an earlier meeting. The main object of this discussion, I take it, is to formulate the opinion of the Society as to the most suitable protection for the X-ray operator. Far be it from me as a mere Colonial—I come from Montreal and other wild parts of Canada—to take issue with any member of this Society, but I certainly do not quite agree with Mr. Schall when he talks about an X-ray stream not affecting the tissues through which it passes if the rays are of a sufficiently highly penetrating order. I know a case in which a man stood for three-quarters of an hour in front of a tube actuated by a static machine and he got a dermatitis. If there is one machine which furnishes a highly penetrating ray with a small milliampèrage, it is the static machine. Such rays, according to Mr. Schall, ought to have passed through without affecting the tissue, but certainly there was a dermatitis.

With reference to conditions in our hospitals: one would hate to criticize either the British War Office or the Canadian Militia Council—we were all rather at sea in these matters when we started on this war. Certainly, in the matter of radiography, we were not advised by the best men on either side of the Atlantic, judging by the results I have seen from Rouen to Calais. There is scarcely any protection. I know personally of two cases of dermatitis among British workers, and I am credibly informed of six others. One hardly knows where to place the blame for not securing the advice of experts, but what I do know is that, considering the state of things over in France, a very sharp line of distinction has to be drawn between the conditions there and the conditions in England. In England the X-ray installations are run by experts, but in France in many cases they are run by men who never did X-ray work longer than a year or so before they were sent over. If the Society would only place on record some categorical statement as to what density of rubber, what thickness of lead should be used, the type of apparatus, the milliampèrage, etc., it would be a great boon. What we want is something *ex cathedra* from this Society to be submitted to the War Office. It is the function of this Society to furnish these statements, to take the lead and give an authoritative dictum as to what should be done. I will tell you what we did in Canada. When we first started our hospitals, we had new apparatus—outwardly very nice, two milliampères going through the tube, 1 m/m of lead covering around the tube—and found it absolutely useless; this we had to scrap. Then on Dec. 19th, we found we were going to France very shortly, and could not get an efficient installation from Canada in time, with the result that we had to go round to the manufacturers in London hurriedly to get a supply. The ideas of the manufacturers on the subject of protection are certainly below what we deemed necessary for the heavy transformer outfits which we got from the United States and Canada. I made my assistants wear heavy rubber 1½ m/m in thickness, and this was fairly satisfactory on testing. It really was a very good rubber protection. Now all our hospitals are protected in the same way. We insist upon having 8-pound lead put round our boxes, and where possible we have two linings of that lead. In our screenings we have 8-pound lead passed round everywhere, and we will not accept a machine that is not adequately lead protected.

The scientific part of the discussion has been exceedingly interesting, but one must not forget the practical application of a discussion of this kind. If only some kind of resolution could be put and carried at this meeting for the guidance of those who are working on X-rays, it would be one of the best things we should have ever done as a Society.

On the subject of ozone, I remember distinctly, having always been a very heavy smoker, that in the old days when the room was full of tobacco smoke I had only to turn on my static machine, open the electrodes just beyond

sparking distance, and the ozone caused by the brush discharge would clear the room of the smoke in five minutes. The tired feeling is due to ozone—it does not make much difference whether the ozone comes from the X-ray terminals or from the terminals of the tube itself.

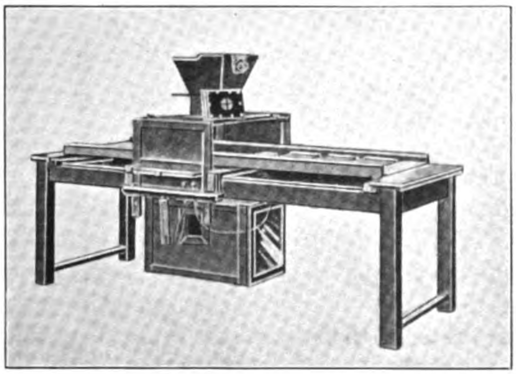
Mr. THOMAS CLARK suggested that when an X-ray tube was excited by an induction coil, a form of radiation might be given off which would be absent if the same tube were excited by an influence machine.

He personally had been exposed and he had also (under medical supervision) exposed patients for very considerable periods without any ill effects.

Using an induction coil, an X-ray burn was produced after three exposures. Would Dr. Russ explain the reason for this ?

Mr. Clark then showed a lantern slide of a couch especially designed to afford protection to the operator.

CLARK'S PATENT FLUORESCENT SCREEN LOCALISER.



The tube is contained in a box lined with lead 3 m/m thick, the ends and top covered with opaque rubber. The diaphragm is fitted in a large metal plate on the top of the box, thus giving extra protection. A special centring device, involving the use of two sets of cross wires, connected with the tube-holder, can be adjusted from the outside.

The metallic indicator to register the movement of the shadow of the foreign body can be set without danger to the operator's hands.

Mr. Clark suggested that the ill effects referred to by some speakers after work in the X-ray room might be due to ozone and oxides of nitrogen, formed by electric discharges from the wires leading to the coil and tube.

Mr. CHARLES A. SCHUNCK wished to ask Dr. Russ where it is considered is the origin of the X-rays that come from the non-luminous hemisphere of the tube.

Screening one of his tubes (three-inch alternative spark gap), the shadow of the hand could be seen at all parts of the non-luminous hemisphere, but no bones were visible ; the radiation appeared to be quite uniform.

Mr. C. R. C. LYSTER : We have listened to a discussion, not only interesting, but of great importance.

The danger resulting from the use of X-rays is well known and has been proved to exist over and over again.

It is certain that this danger is not realized by many who are using X-ray installations, or by those who are directing others to use them.

We all know of the difficulty of getting sufficient numbers of experienced operators at the present time, but this does not justify the appointment of men with little or no knowledge of the risks they are incurring themselves and inflicting on others.

That the right of using X-rays for treating patients, or for the taking of skiagrams should be limited to those who are medically qualified and skilled in their use, or to specially-trained assistants under medical supervision, is the only way in which the dangers can be controlled.

The inspection of installations and a knowledge of the amount of protection afforded is a very necessary help to a successful and safe service.

Dr. SIDNEY RUSS, in replying on the discussion, said that he had found it rather difficult to group together the various aspects of the subject that had been dealt with by the various speakers, and therefore he thought it best to deal with each one individually. Sir James Mackenzie Davidson made an important point when he said that it behoved us to protect ourselves against hard X-rays just as much as against soft X-rays. The fact that the effects

of hard X-rays were so very largely unknown was an additional reason why they should take precautions against them. Mr. Donnithorne's observations seemed to be particularly valuable, because they brought out very pointedly the advantage of shielding the bulb. If the bulb were adequately shielded it seemed to him (the speaker) that a number of the other protective devices became of secondary importance. It was astonishing to see the very small amount of radiation that got through the particular tube-shield Mr. Donnithorne dealt with ; it was much more nearly a question of one part in a thousand than of one in a hundred. Dr. Bailey remarked on the differential action of hard and soft rays, and also, along with Dr. Morton, insisted upon what might be called the small-room evil, and it was quite clear that a great deal of X-ray work was done in quarters which were extremely trying. With regard to Dr. Morton, it was quite easy to understand his cheery optimism. Having himself sustained no damage, he was naturally optimistic about the dangers of X-rays. At the same time he (the speaker) thought that this ought not to blind their eyes to the fact that very often irreparable damage was done—not, of course, to the man who knew what precautions to take and was aware of the dangers, but to the man who came to the work with inadequate knowledge and skill. What Dr. Finzi had to say about the amount of radiation received was interesting, because it showed that if the tube box were not particularly well protected the worker was liable to receive, with the particular installation in question, something like twenty-four pastille doses in the course of a year. He did not see why any X-ray worker should be exposed to this amount of radiation, when it could be avoided by a proper shielding of the tube.

Dr. Harwood Nutt's observations were also interesting, for the reason that he had produced a danger map of his own installation. The glove-cover he had produced might be supposed at first sight to be clumsy, but it was surprising how easy it was to manipulate the hand when thus protected. Mr. Schall raised many questions which would form in themselves very good material for a subsequent discussion. They were really too far-reaching for him to go into on that occasion. It was quite true, as Mr. Schall had pointed out, that Mr. Donnithorne's observations were made with a small electroscope, but it was out of the question for any stray ionization of the surrounding air to get into the electroscope. There was no doubt as to the general correctness of Mr. Donnithorne's conclusion that anything which did enter and cause ionization in the electroscope must have been radiation, and not any stray ionization. Dr. Batten made a very important point when he talked about the dangers of certain chemical substances when these were used in association with X-rays. People who had to do their own developing were often susceptible. Alkaline material especially made the hands very sensitive, and this should be rigorously avoided. With regard to Mr. Clark's question, he could see no reason why, if the potential difference between the electrodes were the same, there should be any difference in the identity of the rays such as he had suggested. With regard to the question raised by Mr. Schunk in his letter, relating to the radiation coming from the back part of the tube, he would prefer not to attempt to answer it without more direct information.

The PRESIDENT said that he was glad to announce that the Authorities of the National Physical Laboratory were prepared to examine protective materials used in X-ray work, impregnated rubber, glass, etc., and to give a certificate of the percentage of hard, medium, or soft rays they absorbed ; this was certainly a step in the right direction, it would be a great gain both to manufacturers and operators to know accurately the absorption value of the material that was being used.

LATE NOTICE, APRIL, 1916.

A resolution, drawing attention to the dangers attending the present practice of Radiography in the Naval and Military Hospitals, accompanied by a memorandum, is in course of preparation, and will be forwarded to the responsible authorities with as little delay as possible.

Price to Non-Members,
4s. *net*; 16s. *per ann.*

THE JOURNAL OF THE RÖNTGEN SOCIETY.

VOL. XII. JULY, 1916. No. 48.

OBITUARY.

SILVANUS P. THOMPSON.

With very great regret we have to announce the death of Professor SILVANUS PHILLIPS THOMPSON, F.R.S., which took place unexpectedly at his home in Chislett Road, West Hampstead, on Tuesday, 13th June, after an illness of only two days' duration.

As is well known, Professor Thompson was our first President, and has always taken the most active interest in the welfare of the Society, his name appears in the business of our last meeting as the proposer of a new member.

Professor Thompson's researches upon the nature of Cathode rays and allied radiations form the subject of several important communications to the Royal Society, and his papers and presence at our meetings will always be recalled with pleasure.

Born at York in 1851, his scientific career, commencing on the attainment of his B.A. degree in 1869, has been brilliant, and embraced the following qualifications and honorary degrees: - B.A., D.Sc. (Lond.), LL.D. Birm., Bristol, M.D. (Konigsberg), F.R.A.S., Reg. Acad. Sci., Soc. Philos. Fbor. Soc. Honor.; F. Phys. Soc., Mem. Inst. Elect. Eng.; Principal and Professor of Physics in the City and Guilds of London Technical College, Finsbury, E.C. He was a recognised authority upon electricity, magnetism and acoustics, and his writings are very numerous. He has held many important positions in the world of science.

His appointment of Principal of the City and Guilds of London Technical College at Finsbury has continued since 1885, where his genial presence and whole-hearted devotion have endeared him to the large number of students who have passed under his training—mnay of whom occupy important positions all over the world.

Apart from his professional qualifications, Professor Thompson possessed a peculiar charm of manner and address that influenced all with whom he came in contact. His death leaves a blank that will be hard to fill, and a pang of sorrow in the hearts of all who had the privilege of knowing him.

A letter of sympathy on behalf of the Members and Council of the Society was sent to the widow by our President and Secretary as soon as the sad news became known.

RÖNTGEN SOCIETY.

A GENERAL MEETING of the Society was held at the Institution of Electrical Engineers on Tuesday, March 7th, 1916, Mr. J. H. Gardiner, F.C.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

MR. JOHN FREDERICK REY, MR. J. W. MASON and DR. W. H. SYME were balloted for and unanimously elected members of the Society.

NOMINATIONS :—

(1) FRANCIS HAROLD RODIER HEATH, 22, Abbotsbury Road, Weymouth.
Proposed by B. H. Morphy.
Seconded by Leonard A. Levy, D.Sc.

(2) Dr. WILL H. EAGAR, Moore Barracks, Shorncliffe.
Proposed by Dr. Howard Pirie.
Seconded by R. Knox.

(3) ERNEST WILBERFORCE HUTTON, L'Hôpital Notre Dame des Gréves, St. Malo, France.
Proposed by Robert Knox.
Seconded by G. Pearce.

(4) FRANCIS DAVIS OWEN KING, 77, Jesmond Avenue, Wembley Hill, Middlesex.
Proposed by R. Knox.
Seconded by G. Pearce.

Mr. ARTHUR C. GUNSTONE read a paper on "The Use of Inverse Current."

THE USE OF "INVERSE CURRENT."

By ARTHUR C. GUNSTONE.

It is my intention this evening to briefly describe to you the results of some experiments which we have recently undertaken with a view to making use of the "Inverse Current" as given by an induction coil operated by a mercury interrupter. I think most of you will agree that one of the most serious drawbacks to the induction coil system, as compared with its rival the high-tension transformer, is the presence of this inverse current, and the necessity of employing some means of preventing that current from passing through the X-ray tube.

In the old days when we were content with currents up to about 5 m.a., this did not present any very great difficulty. Valve tubes were employed, which quite effectively cut this inverse current out and allowed only the current of the correct direction to pass. As, however, the induction coil has been made larger and more powerful, we have experienced more difficulty in dealing with the necessarily increased volume of inverse current, and instead of using one valve tube, it is not uncommon to find two, three or four in series which, beside adding considerably to the running cost of the installation, increases to no small extent, the troubles of the operator.

The X-ray tube itself can usually be relied upon to give enough trouble so far as stability of vacuum is concerned, and if we can dispense entirely with the valve tube, we have made a distinct improvement.

There have been several mechanical devices introduced for the suppression of the inverse current, amongst which I might mention the Mica Disc Valve and the Morton Rectifier, but these and all others of their class only cut out the inverse current.

In the apparatus I have here this evening, the secondary impulse due to the "make" in the primary winding does not oppose that impulse due to the "break," but appears in the same

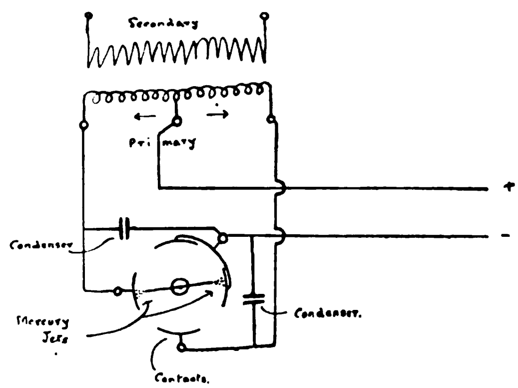
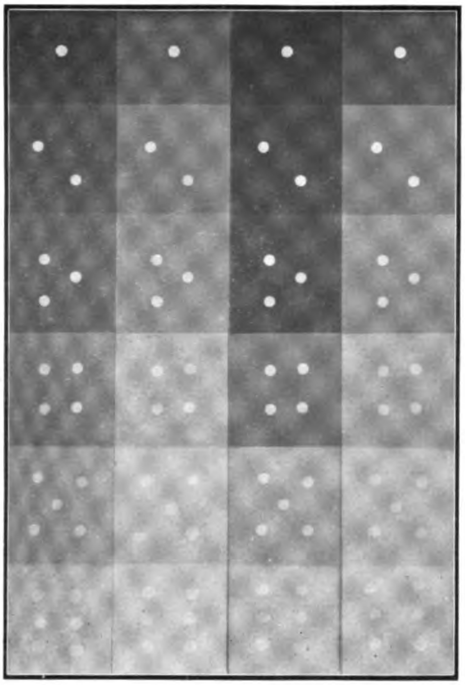


Fig. 1.

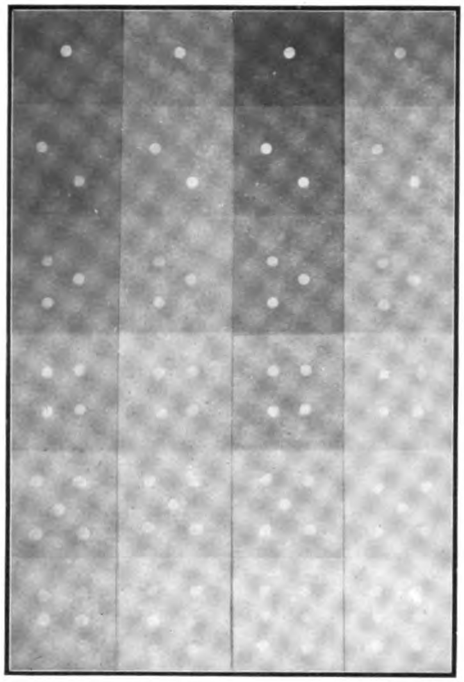
direction, because the arrangement of the primary is such that the magnetization of the iron core is reversed in sign at each successive impulse.

	1. Tungsten.	2. Coolidge.	3. Coolidge.	4. Coolidge.



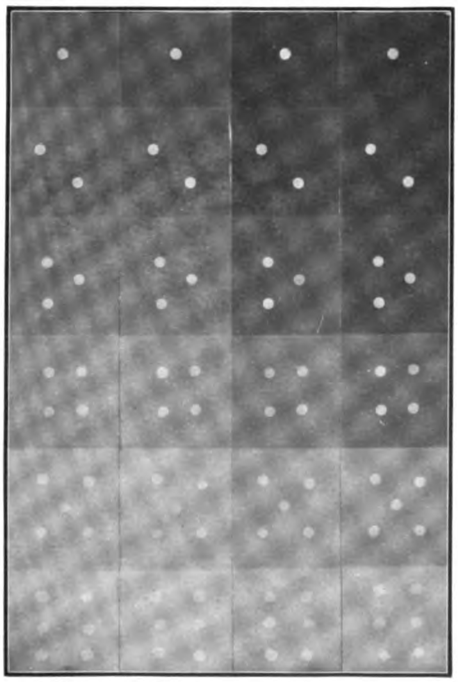
Distance	24 in.	24 in.	24 in.	24 in.
Spark Gap	6 in.	6 in.	6 in.	6 in.
Current	1 M/A.	1 M/A.	1 M/A.	40 M/A.
Time	20 sec.	20 sec.	80 sec.	2 sec.
	20 M/A sec.	**20 M/A sec.**	**80 M/A sec.**	**80 M/A sec.**

	1. Tungsten.	2. Coolidge.	3. Coolidge.	4. Coolidge.



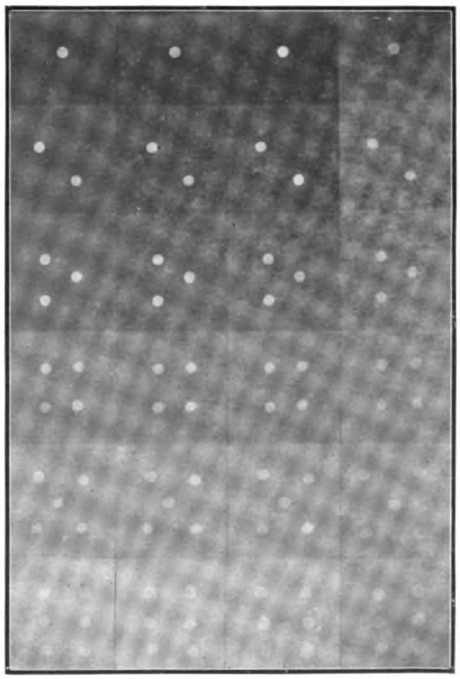
Distance	24 in.	24 in.	24 in.	24 in.
Spark Gap	6 in.	6 in.	6 in.	6 in.
Current	4 M/A.	4 M/A.	4 M/A.	20 M/A.
Time	5 sec.	5 sec.	20 sec.	4 sec.
	20 M/A sec.	**20 M/A sec.**	**80 M/A sec.**	**80 M/A sec.**

	1. Tungsten.	2. Coolidge.	3. Coolidge.	4. Coolidge.



Distance	24 in.	24 in.	24 in.	24 in.
Spark Gap	7·5 in.	7·5 in.	7·5 in.	7·5 in.
Current	7 M/A.	7 M/A.	7 M/A.	12 M/A.
Time	3 sec.	3 sec.	12 sec.	7 sec.
	21 M/A sec.	**21 M/A sec.**	**84 M/A sec.**	**84 M/A sec.**

	1. Tungsten.	2. Tungsten.	3. Coolidge.	4. Coolidge.



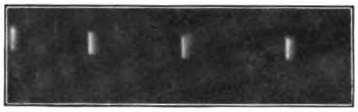
Distance	24 in.	24 in.	24 in.	24 in.
Spark Gap	7·5 in.	7·5 in.	7·5 in.	7·5 in.
Current	3 M/A.	8 M/A.	3 M/A.	8 M/A.
Time	8 sec.	3 sec.	8 sec.	3 sec.
	24 M/A sec.	**24 M/A sec.**	**24 M/A sec.**	**24 M/A sec.**

Experiments with Coolidge and Tungsten Tubes.—By W. E. SCHALL, B.Sc.

PLATE VIII.

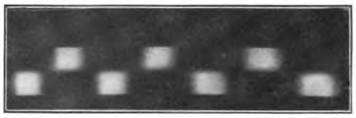
No. 1.—Ordinary System 10 M.A. with valve tube.

No. 2.—The same, but without the valve. (Note the inverse current and long duration of same.)



No. 3.—The New System without valve, 9 M.A. (Note duration of impulse is no greater than No. 1.)

No. 4.—The same as No. 3, but with 25 M.A. (Note the "Make" impulse added on to the "Break.")



No. 5.—High-Tension Alternating Current 50 Cycles. Taken so as to compare duration of the impulses.

"Falling Plate" Photographs of the Oscilloscope Tube.—By A. C. GUNSTONE.

PLATE VII.

For this purpose, the coil is provided with two primary windings with a central tapping, and the interrupter is fitted with four contacts so arranged that when the jet leaves one contact and passes on to the next contact, the current flowing in one primary winding is broken, and then made again in the other winding. The current, therefore, enters the primary at the central tapping and alternately leaves by one of the two remaining ends, thus carrying the iron core through a complete cycle of positive and negative magnetization, very similar to the behaviour of a transformer working on alternating current. We must now study the effect of these primary changes on the secondary windings, and for this purpose we will consider the passage of the jet from one contact to the one next adjacent. For the sake of simplicity, we will consider the core already magnetized by the passage of a steady current and ignore for the moment what happened when that current was first established. The jet is touching one contact and on leaving the contact, the current in one primary winding is broken and an induced current appears in the secondary as the result, next the jet strikes the second contact and makes the current in the other primary winding with the result that another flash appears in the secondary, but this second flash is in the same direction as the first and is not inverse, as is usually the case.

This, of course, follows from the reversal of the magnetization of the iron. Two negatives must make a positive ; the destruction of the magnetization in one direction must have the same effect on the secondary as its creation in the other.

The effect, therefore, of the jet leaving one contact and striking the next is to send two impulses through the tube each in the same direction. We must now study the result of the jet leaving the second contact and striking the third. Obviously, if we now break the current still flowing, the secondary effect will be reversed, as we are destroying the magnetic field of different sign. We must therefore combine with the interrupter, a commutator which will turn over each alternate pair of impulses so as to bring them all into line.

By using four contacts on a double-jet interrupter, we can very easily arrange this by the use of the disc commutator as now used on most of the high-tension transformers. This disc is mounted with the axis vertical and is coupled direct to the spindle of the same motor that drives the interrupter. It is then only necessary to adjust this disc in regard to the position on the spindle in order to send all the pairs of impulses (one impulse due to the " break " and one due to the " make ") through the tube in the same direction. You will notice that in referring to these pairs of impulses, I mentioned that due to the " break " first, although, of course, the current must be made before it can be broken. I mentioned also, earlier in this paper, that for simplicity's sake, we would commence our study of the subject with the primary current on, as by so doing, we need not recognise the fact that during the passage of the jet from one end of a contact to the other, that is, the interval between the " make " and the " break," the high-tension commutating disc would have moved through a quarter of a revolution, and by so doing reverse the connections to the X-ray tube.

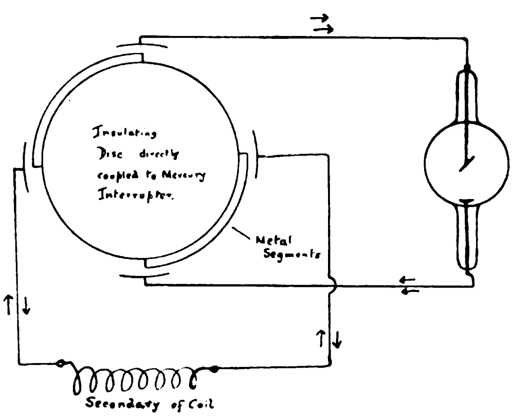


Fig. 2.

We can now follow the secondary effect of one complete revolution. There are four contacts and therefore four gaps around the circle, and during the passage of the jet across each of these gaps, we have a double impulse in the secondary.

Further, these double impulses occur at each quarter of a revolution, and each pair is in the opposite direction to the preceding and succeeding pairs, but this reversal is again balanced by the disc commutator, so that the tube receives four double impulses during each revolution and all these impulses must, of necessity, be in the same direction. The impulse due to the " make " previously spoken of as inverse current, comes along in the same direction as and immediately after the impulse due to the previous " break."

In support of these reasonings, all of which follow from merely theoretical principles, I have made some photographs of an oscilloscope tube when connected in series with an X-ray tube working from the apparatus now before you.

These photographs were taken in a dark-room by means of a falling plate, so that each impression is due to one single impulse only. By way of comparison, I have also made some exposures under similar conditions, but with the ordinary induction coil and mercury interrupter and the use of two valve tubes.

No. 1 is a photograph of an oscilloscope tube connected in series with an X-ray tube worked by a 16-in. coil and mercury interrupter, a valve tube being in series—Current 10 m.a.

No. 2, the same, but without the valve tube. Notice the duration of the inverse impulse.

No. 3 is a photograph taken under the same conditions, but using the special primary and rectifying apparatus I have just described. The same tube was used and the same coil, the two primary windings being simply interchanged. Current was 8 m.a.

No. 4 is a photograph taken under the same conditions, but with a heavier current passing in to the primary. The milliampèrage was over twenty-five. In this photograph the impulse due to the " make " is clearly visible. There is the ordinary sharp flash due to the " break," the same as shown on the preceding photographs, and then there is a slight interval and then the second flash due to the " make," or what generally appears as inverse current. This, I think, is the most interesting slide of the series, as it shows the " make " current passing through the tube in the same direction as the other and contributing to some extent in the production of the X-rays.

No. 5 was taken by means of an alternating discharge in order to show the comparative discharge times of a coil working under these conditions and the high-tension transformer. You will notice that if the negative impulses were turned over and put in line with the others, as is done in the transformer apparatus by means of the rotating rectifier, that the tube would be energized practically continuously with very little or no intervals of rest. It has been suggested that an induction coil working on the lines already described would have the same disadvantage as the transformer on account of the heating of the tube. This I do not think is the case, as an examination of the photographs will show. At medium current intensities, such as is used for screening, one cannot see any appreciable difference between the coil working under ordinary conditions with a valve tube and when working under the new conditions. In both instances, the discharge duration is very small compared to that given by the transformer. By calculating the speed of the plate the moment it reached the lens focus, I have calculated the approx. time of duration.

In the case of No. 5, the duration was 1/100th second, corresponding accurately to the frequency of 50 cycles, which the transformer I used was giving.

$$\left.\begin{array}{ll}\text{No. 1. The ordinary coil system} & = \\ \text{No. 3. The new conditions} \quad \ldots & = \end{array}\right\} \tfrac{1}{500} \text{ sec.}$$

$$\text{No. 2. Duration of the inverse} \ldots = \tfrac{1}{180} \text{ sec.}$$

In the case of number four, which was the new system running at full output, the duration of the impulse is still very small compared to the transformer impulse. The intervals of rest between each successive impulse being a trifle longer than the duration of the impulse itself.

I therefore conclude that even at full output the heating of the tube is considerably less than in the case of the transformer, and at medium

loads, as slides numbers 1 and 3 show, the conditions should be identical with the ordinary induction coil and interrupter.

It is universally agreed that the milliammeter readings on a transformer and an induction coil are not comparable so far as the X-radiation is concerned, and as a further illustration that the discharge of an induction coil working on the lines described still retains its advantage of a "peaky" output, I have made a few exposures of a chest, taking care to give the same exposure, same m.a. readings, same tube and same working distance, using the identical apparatus now before you and a high-tension transformer and rotating rectifier. I have the results here and you can examine them after the meeting. I think you will agree that in the case of the transformer apparatus, you require about double the exposure in milliampère seconds to give the same result as the coil.

By working the induction coil on these lines, you are therefore on as good a footing as the high tension transformer so far as inverse current is concerned, and yet retain the great advantage of an instantaneous discharge and its effect on the X-ray tube. Valve tubes are entirely dispensed with, and the induction coil can be made as large as one desires without being limited as previously by the increased amount of inverse current. In fact, the coil need not be designed to give a minimum amount of inverse current as has been our previous custom, but can now be made, and which is much easier, to give a maximum amount of inverse current. I therefore think that the induction coil operated on this system should compare still more favourably with the high-tension transformer, although for the absolute maximum of output, the latter may probably still hold the field on account of the difficulty in making an interrupter deal effectively with such heavy primary inputs as can be sent into the transformer.

Mr. F. H. GLEW said: I should like to ask Mr. Gunstone what is the diameter of the slit used in taking these photographs, and what is the rate of the fall of the plate. I notice that for each spark you have a continuous effect. It is quite probable that if the slit had been made smaller, we should have seen oscillations in the circuit. The oscillations are analogous to those of a Leyden jar, comprising the whole capacity of the X-ray installation, including the tube itself.

Professor J. T. MORRIS: I should like to ask the reader of the paper whether he makes any alteration in the adjustment of the condenser which is used in connection with the induction coil, because that is known to have a marked effect on the action of the coil. I have been much struck with the neatness of the apparatus. I would suggest that it is just possible that instead of using an open magnetic circuit with the induction coil as is used here, a closed iron magnetic circuit might be used without any of the inconvenience to which in the ordinary way such an apparatus would be subject.

Mr. W. E. SCHALL: The inverse current—that is to say, the "make" current—surely has a much lower E.M.F. than the "break" current—that is, the current in the right direction. Therefore if you pass that through in addition to the "break" current, you will be having two sets of cathode rays, the one fast, the other comparatively slow. In this way two types of X-rays will be generated, one with high penetrating power, and the other with low penetrating power. This is a state of things that we generally wish to avoid. *See note on p.* 5.

Mr. GUNSTONE, in reply, said: The speed of the plate movement was 4·7 ft. per second. With regard to the oscillations, my method was too rough to show these; it was simply made to show the comparative gaps between the two systems. We placed the apparatus in the dark-room, took off the cap of the camera, switched on the current, and released the plate, so that we just got one smudge for each impulse. In reply to Mr. Schall, he is quite correct in what he says about the hard and soft radiation, but, as one of the slides showed, it only occurred when running at full output that the inverse current got through at all, so that the disadvantage of

heating the tube when used for deep-seated therapy is scarcely likely to occur. If the tube is hard, such as would be used for this purpose, the inverse current would not get through at all. In that case, there will be no difference between this system and the ordinary system. If the inverse current does get through, we shall get hard and soft rays. In reply to Professor Morris, the condenser is the same as we generally use for the ordinary coil. We tried increasing and diminishing it, but it does not make very much difference.

LETTER TO EDITOR.

Mr. B. H. MORPHY: A number of interesting points arise out of Mr. Gunstone's valuable paper. One would like to see an oscillogram of the voltage wave of the apparatus, or, better still, the spectrogram of the resultant X-ray beam taken either by the Bragg crystal method or by absorption.

A most interesting paper has recently been published in the States showing that the applied voltage affects the wave length of the X-ray beam to an almost unexpected degree, and that low voltages comparable to the "inverse voltage" of a good induction coil are practically valueless for producing rays of a useful wave length. This leads one to suspect that the only result of applying the "inverted inverse" to a tube would be to produce heating without effecting an appreciable increase in the yield of X-rays.

If this be not the effect, it seems likely that the direct voltage peak is flattened while the inverse is magnified, thus approaching the transformer wave, which is generally reckoned inferior to the coil wave for most purposes.

It is rather difficult to predict the wave form from the short description given, but it appears that the effect is practically that of a double-frequency interruption of half the amplitude which would exist in an ordinary coil. I should be very glad if Mr. Gunstone would enlighten us further on this point.

The milliamperemeter readings given during the demonstration were very interesting, but not convincing, as to the value of the current from a productive point of view. To illustrate this point it may be recalled that if a coil be connected the incorrect way to a coolidge tube, 50 m.a. may readily be passed through it, but the rays will hardly penetrate the backing of a screen or the frame of cassette, though the current will soon heat up the tube.

Mr. A. C. GUNSTONE: In answer to Mr. Morphy's letter, I am sorry I am unable to furnish the spectroscopic tests which he mentions. His criticism appears to me to be identical with the point raised by Mr. Schall at the meeting during which I brought the apparatus before the notice of the Society.

If the secondary impulse due to the "Make" passes through the tube in the same direction as that due to the "Break," it is perfectly true that the X-radiation due to it will be less penetrative than that due to the higher voltage impulse caused by the "Break," but on the other hand, I think it will be generally admitted that the tube will suffer very much less than it would do, if the "Make" impulse was allowed to pass through in the wrong direction as "inverse," as it very often does in the ordinary system of working.

With regard to the heating of the tube by the inverse current the photographs which I showed were made with the express purpose of gauging this heating.

These slides quite conclusively prove that with a medium tube and currents up to 9 milliampères at least, the inverse current did not approach a sufficiently high voltage to pass through the tube at all. The discharge was exactly the same in duration and intensity as that of the discharge of an ordinary coil, with the same secondary current.

I think one would never use higher currents than this, either for screen work or deep-seated therapy, in which case the heating of the tube would obviously be no greater than that which would result from an ordinary coil installation.

The impulse due to the "Break" is exactly as usual, and is not flattened at the peak as Mr.

Morphy suggests. In one revolution of the interrupter, four "Break" impulses are produced, all identical in power and pressure to that given in the ordinary coil, and, in *addition*, there are four "Make" impulses, provided the tube is not "hard."

Only when the tube is soft and the coil is running at full output does the "Make" impulse materially heat the tube, and even then, as my photographics show, the heating is very much less than would be the case with the transformer. The ordinary interrupterless transformer is very successful for fast radiography, but it has great disadvantages for screening and therapy, and for these branches of our work the system I have explained is quite equal to the ordinary coil.

I do not claim that the "Use of Inverse Current" is any very great advantage; perhaps the choice of the title of my paper was unfortunate. I did not set out, in designing the apparatus, to find a way of using it, it was its ellimination which I desired, but I found it much easier to use it than to successfully cut it out.

No induction coils, no matter what their design or make, have ever been run at their full output, because when this is attempted, the inverse current has become almost unmanageable.

Several times have I been told by users of all different makes of coils that whenever they try to exceed 10 m.a., the needle of the meter begins to read the wrong way!

A GENERAL MEETING of the Society was held at the Institution of Electrical Engineers on Tuesday, April 4th, Mr. J. H. Gardiner, F.C.S., President, in the chair.

The minutes of the previous meeting were read and confirmed.

The following were unanimously elected members of the Society:—

Mr. FRANCIS HAROLD RODIER HEATH.
Dr. WILL N. EAGAR.
Mr. ERNEST WILBERFORCE HUTTON.
Mr. FRANCIS DAVIS OWEN KING.

NOMINATIONS:—

(1) P. J .NEATE, 59, Frognal, Hampstead, N.W.
Proposed by St. George Caulfeild.
Seconded by R. Knox.

(2) HAROLD JAMES STENNING, 164, Upland Road, East Dulwich, S.E.
Proposed by B. H. Morphy.
Seconded by Dr. Leonard A. Levy.

(3) JOHN ROWE DUTTON, Winrath, Winscombe, R.S.O., Somerset.
Proposed by Dr. Harold E. Nounton.
Seconded by R. Knox.

The PRESIDENT then called upon Dr. HARWOOD NUTT to move a resolution that was mentioned at the previous meeting, the outcome of the discussion on protective measures for X-ray workers.

Dr. HARWOOD NUTT said that he thought it did not require many words from him to convince the members of the Society of the necessity for this resolution, after the discussion they had had at the last two meetings. He must admit that it was a delicate task the Society had set itself, but it was a task which he thought they could not shirk; some of them when on active service at the front had seen cases of burns which they could authenticate.

He would like to ask those who had cases of this kind to give particulars of them to the Honorary Secretary of the Society.

The resolution would be of little value unless it could be strengthened by cases. He would also add that a resolution of this kind, asking for competent inspection of X-ray apparatus and installations would not be necessary if the members of the Society were the only workers in this field, but unfortunately they were not, and for that reason this resolution had been brought up, so that those operators who had only a dim idea of the dangers of working X-ray installations, should benefit. Many of these workers wanted advice. Ordinarily it was difficult to penetrate officialism with isolated complaints, and improvements were not added to installations as promptly as they should be.

For the sake of these workers he thought they ought to back up this resolution unanimously, and in the confidence that they would do so, he submitted it.

Mr. SIDNEY RUSS formerly seconded the resolution, and it was carried unanimously.

The resolution and the memorandum that accompanied it was as follows :—

RESOLUTION.

"We, the Members and Council of the Röntgen Society, view with some concern the present condition of the X-ray examination of patients in His Majesty's Naval and Military Hospitals, in view of the fact that a number of installations—some of which we believe are defective in their means of protection—are in the hands of inexperienced X-ray workers who do not fully realize the attendant danger.

"We, as a Society, would suggest to the responsible authorities that every installation both at home and abroad should be inspected by experienced radiologists in order that efficient means be taken to ensure complete protection to patients and operators."

MEMORANDUM.

The Council of the Society deem it expedient to bring before your notice the following points in support of the foregoing resolution and for its further elucidation.

The Society is in possession of the following facts :—

1. That X-ray burns are being produced amongst workers.
2. That many of the outfits sent out for the Government are not adequately protected.
3. That the necessary protective measures, such as gloves, aprons and lead glass screens, do not always come up to a sufficiently high standard.

In order that necessary precautionary measures be taken to ensure the safety of the operator and patient, the Council begs to suggest that :—

1. A rigid system of inspection be undertaken by the Authorities for :
   (*a*) The maintenance of a general and particular oversight on the health and well-being of the X-ray worker.
   (*b*) For the detection of faulty installations.
   (*c*) For the advice and guidance of inexperienced X-ray workers.
   (*d*) For reporting upon every installation under the authority of the Government.
   (*e*) To assist and advise the authorities in keeping up and adding to installations under their care, in view of greater efficiency.
2. That a Medical Radiologist should collaborate with a Physicist in this work of inspection for the following reasons :—
   (*a*) That this is primarily a medical question and that a medical man would be brought into contact with the Medical Radiologist in charge.
   (*b*) That he would be competent to detect pathological lesions in their early stages and give advice thereon.
   (*c*) That it would be his duty to collect material of medical interest for the Government and to keep such necessary records.
   (*d*) That he should also advise on other matters of importance, such as ventilation of the X-ray room and the aseptic condition of the couch and its accessories.
3. That a Physicist collaborate with the Medical Man for the following reasons :—
   (*a*) That it is his duty to report upon the physical properties of the protective rubber, lead glass, and other protective measures of the installation.
   (*b*) That it would be his duty to collect material of physical interest for the Government and to keep such necessary records as may be deemed advisable by the Authorities.

Mr. P. J. NEATE described and demonstrated

A CHRONOGRAPH CONSTRUCTED TO WORK WITH THE ELECTROSCOPE.

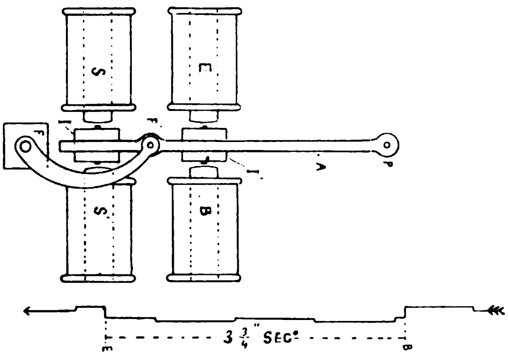
He said that a few weeks previously he had had a conversation with Captain Phillips upon the absence from trade catalogues of any chronograph suitable for accurately measuring intervals of time up to five or six minutes. Captain Phillips told him that such an instrument would be useful for measuring the duration of electrocsope readings. By scraping together a few oddments in the way of ready-made materials, he (the speaker) had constructed the arrangement which was exhibited before the meeting. The prime mover and drum are parts of an old gramophone, and by altering the weight of the governor balls, it has been modified so as to run at the pace required.

The general scheme of the apparatus is to draw a spiral trace on the cylinder with very short sharp excursions to right or left at the end of each second, making a line just sufficiently staggered to render the counting of seconds easy.

As ten seconds go to one revolution of the drum, counting is further facilitated.

Instead of using a supplementary pen for recording the start and finish of an experiment, the same pen has a much larger stagger superimposed on its other motions by a supplementary device.

The following diagrams show these movements and the means by which they are accomplished.



The pen P and two soft iron armatures I, I^1 are carried in an aluminium frame A, the weight of which is supported by the arbour F^1 attached to an arm free to swing on the fulcrum F, which is fixed to the magnet frame, the latter being fixed to the travelling carriage of the gramophone in place of the usual sound box.

Before the commencement of a period to be measured the magnet E is energized through an ordinary two-way electric light pear switch held in the experimenter's hand. It therefore pulls its armature I^1 away from B and holds it firmly against its slightly pointed pole.

Magnets S, S^1 are alternately connected with the electric current by a seconds pendulum actuating two spring contacts. Their armature, I, oscillates at intervals of one second between the poles of S and S^1, with pole-piece E acting as a fulcrum (F^1 being only used as a sort of floating support).

The result is to move the pen about half a millimetre to the right or left each second, and to draw a slightly staggered line on the drum.

When the period of time to be measured commences, the experimenter reverses his pear switch by lightly rapping the end of the push stem on the table, and then turns it round in his hand ready to again reverse the current at the stop. This action de-magnetizes E and magnetizes B, throwing the pen suddenly over about 2 mm. to the right.

Thenceforth the pole-piece B becomes the fulcrum and the small rhythmic staggers continue in a line parallel to, but to the right of, their original direction.

At the end of the period of time to be measured the experimenter (having already inverted his pear switch) taps its push stem again on the table, when B lets go and E takes hold, thus throwing back the rhythmic line to its original course.

By the side of the diagram sketch is a full-sized reproduction of the trace or record produced, B and E being the beginning and end of an experiment of 3¾ seconds duration.

The errors of the instrument may be grouped into four classes.

1st. The pendulum error, which, being of the order of a minute a week, is negligible.

2nd. The time lags of B and E in their response to the switch. These must be practically identical and cancel out.

3rd. The personal time lag of the experimenter, which should be an equal amount at the beginning and end, as the muscular action is identical. These also should cancel out.

4th. The error involved in measuring the two fractions of the first and last seconds. As each second measures nearly an inch on the diagram, an error of 1/40th of an inch in measuring each fraction could not represent more than 1/20th of a second.

There is every probability therefore of the machine recording the true time within 0·1 second for any period from about 2 seconds to 6 minutes.

Mr. Neate pointed out that the instrument shown was very roughly made, but was so designed that no portion needed any accuracy of dimensions or fitting, except the length of the pendulum, which was a perfectly simple matter of regulation.

Having had some experience with the instrument, he would prefer to drive the drum train by a weight instead of a spring, though as a matter of fact errors in the speed of the drum do not involve any loss of accuracy.

Mr. B. H. MORPHY and **Mr. S. R. MULLARD** then read a paper (accompanied by a demonstration) on

THE ENCLOSED TUNGSTEN ARC AS A SOURCE OF ULTRA-VIOLET LIGHT.

The lamp we are introducing to you this evening is intended to meet the demand, amongst therapeutists, for a convenient source of ultraviolet rays.

There has recently been a great deal of talk concerning Tungsten Arcs and one using special electrodes has met with a great deal of publicity.

On the table is a simple arc-holder carrying two of these electrodes, which will now be switched on. Several distinctive features can be noticed. Firstly, a very intense light is obtained. Secondly, the arc does not remain at all steady, but splutters continuously. Thirdly, dense fumes of a tungstic oxide are generated.

This arc will not bear extension beyond about 5 m/m, and as the electrodes eat away very rapidly, adjustments have to be made continually.

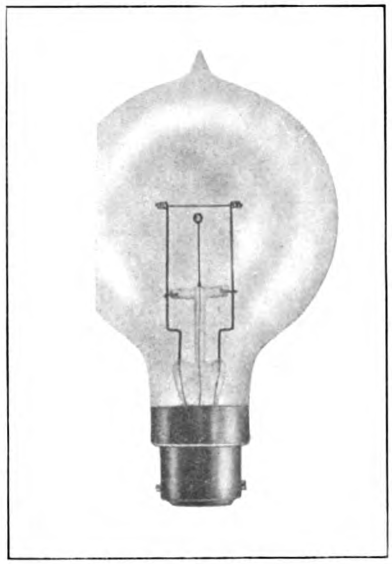
A great alteration can be effected by substituting solid tungsten for the composite friable electrodes.

The second experiment shows these electrodes substituted for the original ones and you will observe that a very much steadier arc flame is obtained. The splutter is completely avoided and the arc can be drawn out to over one centimetre before it is extinguished. There is the same formation of tungstic oxide, but the operation of the arc is very considerably improved.

The first illustration shows spectrograms of these two arcs, and it will be noticed that there is very little difference between them.

Both these forms have great objections which are fundamental to an open arc, and the development of the enclosed tungsten arc in the Ediswan Laboratories suggested a possible means of producing the same rays in a much more convenient manner.

The Pointolite Lamp, which was the result of the early experiments, was in a glass bulb, and if the ultra-violet rays were to emerge it would be necessary to enclose it in quartz, as glass absorbs all rays of short wave length.



Pointolite Lamp.

This Quartz Pointolite Lamp is before you this evening. A short description of the lamp follows, but those who would like fuller details may find them in the paper on the Glass Model communicated to the I.E.E. on Dec. 1st, 1915, by Gimingham and Mullard.

The lamp is the result of a series of most interesting experiments, and is the very first really successful enclosed arc lamp. It is well-known that ordinary supply voltages are insufficient to start an electric arc through even the smallest airgap, but that once the current is started, an arc may be maintained by quite low potentials. It is the initial striking which has been the chief stumbling block in the way of a totally enclosed arc, and the ingenious

method in which this is overcome is the chief feature of the Pointolite. When a filament of Tungsten or other material is raised to a high temperature, electrons are liberated which, through ionization by collision, form an electrically conducting path. In this lamp a filament, which also forms a negative electrode, is heated by a shunt circuit, which is closed temporarily

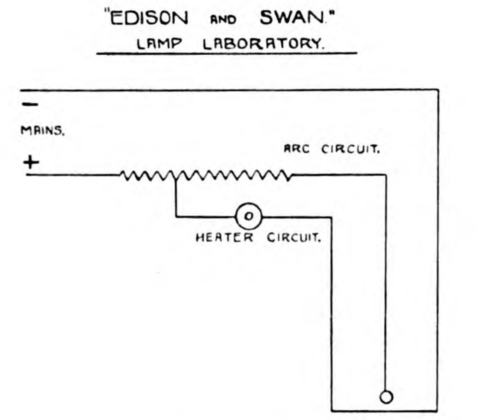


DIAGRAM OF CONNECTIONS OF ARC INCANDESCENT · LAMP.

when the lamp is switched on. The ionization which takes place permits a current to pass to the tungsten globule which forms the other electrode. This starts the arc, which can then be maintained indefinitely. On the screen an image of the arc in action is projected and several interesting features can be noticed. As soon as the filament is heated, the arc starts, the heater circuit is opened and the arc is maintained, the globule gradually rising in temperature until it is at a very bright white heat. As the globule gradually gets hotter, the action of the small expansion strip, which consists of molybdenum and nickel copper alloy, can be noticed. This strip is inserted between the positive electrode and the lead stem, and its action causes the arc to move from the original striking point to a position further along the ionizer. This is a great advantage, lengthening the life of the lamp and enabling the arc to be struck readily during the whole of the life, as naturally in the nieghbourhood of the position of continual arcing, the filament is liable to become inactive as regards ionization. Figs. 4, 5 and 6, show spectrograms of the new lamp,

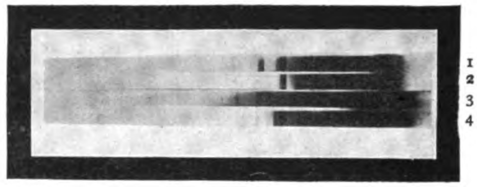


Fig. 4.

both in glass and quartz. Fig. 4 shows the comparison of the quartz lamp with the glass. Different exposures are given. The first and third spectrograms are for the quartz lamp, while the second and fourth are for the glass. The absorption due to the glass is easily noticed.

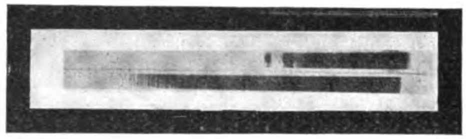


Fig. 5.

Fig. 5 compares the Quartz Pointolite with the Open Tungsten Arc, and at first the result shown seems very disappointing, but it was soon realized that this fact was due to the enclosed form not being run at a sufficiently high temperature.

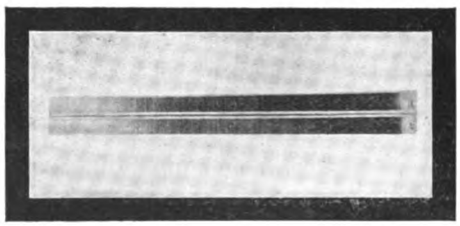


Fig. 6.

Fig. 6 shows the effect of running the lamp at a higher current than would be used for ordinary lighting work. The great increase of ultra-violet light can be remarked. The life of the lamp is, of course, diminished by the over-running, but when run at double its normal candle-power, a life of at least 200 hours may be reasonably expected. This is, of course, not so high as might be wished, but the lamp still

works out at only a small fraction of the cost per burning hour of any open Tungsten Arc.

In conclusion, the authors wish to express their indebtedness to Dr. Russ and Dr. Fox for their kindness in allowing reproductions of spectrograms taken by them.

In order to show the very great increase of ultra-violet rays when the temperature of the arc is raised, a series of spectrograms was taken with various currents, and the results are shown in Fig. 7.

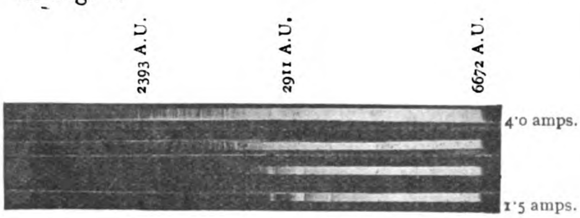


Fig. 7.

This shows most clearly that when the arc current is run up to approximately 4 ampères the output of ultra-violet light is of the same order as from the open tungsten arc, and exactly similar therapeutic results may be expected from it without the great objections of unsteady working and the production of white fumes.

Major ROBERT WILSON said that he was very much interested in the question of ultra-violet light, more particularly with reference to the treatment of wounds at the front. The French were using quite extensively some form of ultra-violet light for the treatment of these granulating and suppurating wounds. His own investigations led him to confirm the statement that the ultra-violet light from striking an arc in tungsten vapour was of much shorter wave-length than had hitherto been obtained. From the medical point of view, he was not able to say whether it was very important that they should have these extremely short wave-lengths, except from analogy, but the important thing was that they should be getting a large quantity of ultra-violet rays. Now, the point brought out by Dr. Russ was important. The "Pointolite" lamp was a tungsten arc, but was burning in a nitrogen vapour, and the question was whether that gave one the same quantity of ultra-violet rays as was given by the open arc. About a fortnight previously he had suggested the design of a lamp to a manufacturer, and when he first heard of the enclosed Tungsten arc, he wondered whether his idea had been anticipated. He thought that if they had a lamp which would give a powerful arc, using say not more than 6 ampères, supplied by a small dynamo when the current from the mains was not available, they would certainly have a means of therapeutic treatment. The principle of the lamp was simple. It consisted of the use of tungsten and of carbon, which might or might not contain calcium tungstate or other high atomic weight core, enclosed in a quartz cylinder, which would prevent the vapour reaching the room. In this way the effect of short wave-lengths and also of quantity of radiation would be obtained. The open Wolfram arc required a great deal of attention, melted away very quickly, and it cost a great deal of money to buy the electrodes. His own idea, therefore, was to have a tungsten base as one electrode, and a carbon as the other. This would cost very little for carbons, and would give the same result as tungsten vapour. Such lamps, he thought, had a field in the treatment of certain kinds of wounds which were seen at the Front and which came before them in the base hospitals in England, as well as in the treatment of certain skin affections, for which larger and more expensive lamps had previously been used.

Mr. C. A. SCHUNCK asked whether the lecturer had experimented with carbon electrodes impregnated with uranium nitrate and ammonium molybdate. These, known as Jones' electrodes, are used with the spark as the source of light in photographing absorption spectra in the ultra-violet region, the lines being so numerous and close together that, with a slightly widened slit, an almost continuous spectrum is obtained, extending throughout the spectrum to wave-length 2200A° about. No doubt the same continuous spectrum would be obtained using them with the arc, and if it was a question of

expense in ultra-violet light treatment, these electrodes might be found to cost less than the tungsten ones, and perhaps prove more efficient.

Dr. SIDNEY RUSS said that he was tempted to say a word on the question of tungsten radiation. It was clear that there was a great difference in the spectrum of the "Pointolite" lamp and the open arc of tungsten. The incandescent vapour of the open arc of tungsten gave a beautiful series of lines which ran right up to 2,300 Ångstrom units. The "Pointolite" lamp was a tungsten arc in nitrogen vapour, and it was interesting to find that the incandescent nitrogen was playing its part as well as the tungsten, and thus one had the superposition of two spectra. Mr. Morphy had pointed out that the range of radiation as between the two sources was quite different. With the "Pointolite" lamp one did not get the very short wave-lengths. He did not think the question as to what wave-lengths had any medical value could be settled quite so easily as had been suggested. This was a matter difficult to determine, because although such radiation was very easily absorbed by the skin, it might be that from the radiations of short wave length there resulted a considerable physiological effect.

Dr. E. P. CUMBERBATCH said that this was not the time for discussing what radiations were most valuable therapeutically, but he certainly thought that Mr. Morphy had begged the question in saying that there was no value in the shorter wave lengths in the ultra-violet. Nobody knew the *modus operandi* of ultra-violet radiation in inducing physiological effects. It was true that Dr. Sequeira, at a meeting of the Royal Society of Medicine, did say that ultra-violet radiation of very short wave-lengths would not penetrate objects which were opaque to ordinary light; but it must not be thought that necessarily there was no effect. One could not make any definite statement about rays of shorter or of longer wave-length, and it was possible that any therapeutic value which the Tungsten Open Arc possessed was to be attributed to the very large number of waves of shorter length.

Professor J. T. MORRIS said that when working the enclosed arcs, the most usual trouble one would find, he imagined, was that a deposit on the quartz would very rapidly cut off part, at any rate, of the radiation. He had had the opportunity of working with the "Pointolite" lamp, and had been much struck with its constancy. As previous speakers had mentioned, there was no doubt that the constancy of radiation obtained when working at a definite current was remarkable.

The PRESIDENT said that he would like to ask one question. What was the "Simpson" arc? Was it pure tungsten? He expressed the hope also that the authors of this very interesting communication would give them the wave-lengths of the enclosed arc and of the open arcs respectively in figures, so that they might have something more than spectrograms to go upon. He would like also to make a suggestion —he did not know whether the authors of the paper had tried it—namely, that metallic uranium might be a valuable metal for this purpose. The spectrum was exceedingly rich in ultra-violet light, the lines were very close together so as almost to approximate to a continuous spectrum, and extended very far up into the ultra-violet.

Mr. MORPHY, in reply to the questions, said that Mr. Schunck had asked whether he had used ammonium nitrate and various salts impregnated, in connection with the open arc. They had not actually taken these tests, because what they were after was a convenient form of enclosed arc. Wherever one had carbons, impregnated or not, one had a troublesome arrangement, no matter whether enclosed or unenclosed, and it was quite impossible to work an enclosed carbon arc in the same way as the enclosed tungsten arc worked. There was no enclosed arc until the "Pointolite" was brought forward. Dr. Russ had drawn attention to the difference between the spectrum of the closed and open arcs. The difference was a wide one, and it was a matter for the therapeutists to decide which of the lamps was the more useful. In reply to Dr. Cumberbatch, he was very sorry

if he had seemed to presume to judge on therapeutic questions. He merely mentioned what was stated by Dr. Sequeira, that these rays of very short wave-length did not penetrate the skin. What actual rays were needed they (the authors of the paper) could not presume to say, and they left the matter to those who were better qualified to pronounce an opinion. He only hoped that it would be found that, as this was a much more convenient form of lamp than the open arcs, its practical advantages would outweigh any deficiency in other respects. He was interested to hear that Major Wilson had been producing an enclosed tungsten arc. Professor Morris's point with regard to the deposit occurred to him immediately Major Wilson had spoken ; if the treatment lasted more than a minute or two, trouble in this respect was likely to be experienced. The enclosed arc which they demonstrated that evening gave a large quantity of ozone, owing to the ultra-violet rays, showing that it must have a certain value in any case in the treatment of wounds by ultra-violet light. Mr. Glew had asked for any comparative tests with the iron arc spectrum. This was very similar to the tungsten arc, but whether or not it was exactly the same he could not say. He believed it was very close to the tungsten, but it would not be possible to work it by the same methods. Sir James Mackenzie Davidson had referred to the use of the " Pointolite " lamp for photographic purposes. It had a very wide field in projection work, and many doctors also found it of use for surgical lamps, being superior to the Nernst lamp, which was used so extensively up to the time of the war. He could confirm what Professor Morris had stated with regard to the regularity of the arc. To any electrical engineer it was wonderful how this arc worked. As to alternating current, successful lamps had been made experimentally for working on this circuit, and after the war Messrs. Edison & Swan hoped to bring them out as commercial articles. The President had asked with regard to the material of the Simpson arc. He had not had it analysed. It was a tungsten compound together with certain other materials. From the appearance of the arc when struck, he imagined that silicates were were present. He would be pleased to give full details regarding the wave-lengths of the enclosed tungsten arc spectrum later. A great many metals had been tried in connection with the manufacture of this arc, and tungsten seemed to be the only one really available. Others which had been tried had proved unsuccessful. In conclusion, he would like to offer the joint thanks of the authors both to Dr. Russ and Dr. Fox for permission to use spectrograms taken by them, and personally he would like to say that he had to take none of the credit for the experimental work, which was Mr. Mullard's ; all he had done was to suggest the quartz bulb for this special purpose.

A vote of thanks was accorded by acclamation.

EXPERIMENTS WITH A COOLIDGE TUBE.

By W. E. SCHALL, B.Sc.

Gentlemen,—During some recent tests made on a Coolidge Tube with a view to ascertaining the degree of definition of bones, etc., which it would produce on a photographic plate, I came across a curious state of affairs which I bring before you this evening, and for which I hope we shall find an explanation.

The photographs which were made were of a thorax, and taking into account the focal distance, penetrating power of the rays, and the milliampèrage of the tube, I thought that the exposure given was unduly long.

It occurred to me that perhaps for some reason or other the rays from the Coolidge Tube do not affect the photographic plate in the same way as those from an ordinary platinum or Tungsten Target Tube. Experiments to determine this point were thereupon made, and I may say at once that I did not obtain a definite result as to whether there is or is not a difference in actinic effect on a plate between ordinary tube and Coolidge Tube rays. Another point, however, became apparent, and is very marked on all the plates that were made.

In referring to an X-ray exposure we have been accustomed to multiply the current which passes through the tube by the time for which it passed and to quote the result as so many milliampère seconds or milliampère minutes. That is to say that we can produce the same density on a photographic plate by passing one milliampère through the tube and exposing for ten seconds, or by passing two m.a. for five seconds, or five for two seconds, or ten for one second.

This is, of course, analogous to our way of expressing work done as the product of a force multiplied by the vertical distance through which it acts—the result being known as foot pounds. In the case of X-rays the point is easily verified by simple experiment, but we must bear in mind that a high current in one and the same tube generates rays of greater penetrating power than a low one.

The experiments which I made seem to reveal that fact that in the case of the Coolidge Tube this product of current and time does not hold good.

The object photographed was the same in each case, namely, an aluminium ladder with six steps, the thickness of which increases in geometrical progression. Each plate is divided into four parts, and, of course, while any one part was being exposed, the other three were carefully screened with thick lead sheet.

The first photograph shows you an attempt to detect the difference in action on a plate of the rays from a Tungsten Tube and a Coolidge Tube. I. is the Tungsten Tube result with 1 m.a. and 20 seconds, and II. is the Coolidge Tube result with the same conditions exactly. You observe that II. is lighter than I. III. is an attempt to get an exposure from the Coolidge Tube equally dense to the Tungsten Tube I. The current for III. was the same, but the time was 80 secs. III. you see is much darker than I. IV. is a Coolidge Tube exposure with the same milliampère seconds as III., but with the factors 40 m.a. and 2 seconds, instead of 1 m.a. and 80 seconds. You see that IV. is much lighter than III., and much more like I. and II., although its m.a. seconds value is four times as much as either of these.

The next slide shows you the same result using a larger current. I. is the Tungsten Tube 4 m.a., 5 seconds. II. is the Coolidge Tube under the same conditions, whereas III. is the Coolidge Tube 4 m.a., 20 seconds, and IV. is the Coolidge Tube 20 m.a., 4 seconds. Again II. is a little lighter than I. and IV. much lighter than III.

Slide three shows you a similar result with yet another current. I. is the Tungsten Tube 7 m.a., 3 seconds. II. is the Coolidge Tube under the same conditions. III. is the Coolidge Tube 7 m.a., 12 seconds; and IV. is the Coolidge Tube 12 m.a., 7 seconds. The only difference from the previous plates is that II. is a little darker than I. in this instance.

These three plates are the best of about a dozen which were taken and which all showed exactly the same result.

The last slide is perhaps the most interesting of all. In this I. and II. are Tungsten Tube and III. and IV. the Coolidge Tube—all with the same number of m.a. seconds exactly.

I. and III. are 3 m.a., 8 seconds, and II. and IV. 8 m.a., 3 seconds. You see that I. and II., *i.e.*, in the Tungsten Tube case, the alteration in the factors of the m.a. seconds product makes no difference, whereas in III. and IV. in the Coolidge Tube there is a considerable difference, and in each Coolidge case the photograph taken with the smaller current is the denser one.

Capt. G. W. C. KAYE thought that Mr. Schall's experiments sufficiently proved, first of all, that neither of the methods he had chosen was of very much use for measuring X-rays. The success of the photographic method rested purely upon the fact that if the rays were capable of exciting the characteristic rays of bromine or silver, then the image was recorded, and if not of a kind to do that, then they would get inadequately recorded. All that Mr. Schall's experiments had proved was that the rays in one set of experiments were capable of fully exciting the X-ray plate, and in the other set of experiments were not so capable.

DISCUSSION.

Major WILSON asked the nature of the plate used, and particularly whether Wratten and Wainwright's plate, which was supposed to contain the salt of a heavy metal, had been used, and with what effect.

Mr. B. H. MORPHY asked whether the results had been produced on an installation with an induction coil or a transformer outfit, because there was a great difference between the results obtained with the one and with the other.

Dr. BARCLAY confirmed the last speaker's statement. He found the results with different outfits vary very greatly, according as to whether a coil or a transformer was used. In one case with the Snook he was able to get a 4-in. spark and 2 milliampères, while with the coil a 9-in. spark gap under the same conditions was almost unworkable.

Dr. FINZI asked the nature of the lantern plate used.

Mr. SCHALL said, in reply to Major Wilson, that the plate used was an Ilford X-ray Plate, and that in regard to the question of the salt in the plate he had also made a test with barium platino-cyanide pastilles, because it struck him that perhaps if there was anything in the theory he had about it, a heavier salt would have a different ratio. He found that the pastille always turned more quickly with the Coolidge Tube than with the Tungsten Target Tube.

He agreed with Dr. Barclay and Mr. B. H. Morphy that different installations would have very different effects. In order to overcome this variability he used a 12-in. coil with mercury interrupter throughout. The cause of the difference of effect of a 12-in. coil or an interrupterless high-tension transformer is, of course, due to the fact, which is now well recognised, that the form of waves of the secondary discharge is different for the two types.

He would like to agree very heartily with Dr. Kaye that our methods of measurement are poor. It was the bare truth that we have no satisfactory means of measuring either the penetrating power or the dose of X-rays. At the same time, until we have such a method, we must make use of the tools at our disposal.

On the other hand, he could not agree with Dr. Kaye that in the present instance the method was a poor one. In the first place, the conditions under which each plate was taken are identically the same, and therefore each photograph was subject to the same advantages and disadvantages. And, in the second place, the whole point of the paper was, that whereas an alteration of the number of milliampères in the milliampère second quotient produced no change in the effect of the tungsten tube rays on a photographic plate, such an alteration does produce a considerable change in the case of rays coming from a Coolidge tube. Dr. Kaye's last remark, therefore, would seem to agree that the experiments had shown that such a state of affairs does really exist.

I have searched for an explanation of these results and have arrived at the theory which I indicated on the occasion of the last meeting. I think that the radiation of the Coolidge tube is very heterogeneous, and that an increase in the number of milliampères passed through the tube greatly increases the hardness of the rays. I believe, in fact, that owing to the way in which the passage of cathode rays is facilitated, namely, by a red-hot cathode, a slight increase in E.M.F. on the poles of the tube, such as would be necessary to increase the milliampèrage from, say, 3 to 8, is sufficient to increase the speed of the cathode rays so much that an X-radiation results with a penetrating power somewhere in the neighbourhood of that of gamma rays. Hence the low current photograph is taken with soft rays which are absorbed by the emulsion, and therefore produce an actinic effect, whereas the large current rays pass through the emulsion to a larger degree, and therefore produce less photographic effect.

RÖNTGEN SOCIETY.

A GENERAL MEETING of the Society was held at the Institution of Electrical Engineers on Tuesday, May 2nd, 1916, Mr. J. H. Gardiner, F.C.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

The following were unanimously elected members of the Society :—

Mr. P. J. NEATE.
Mr. HAROLD JAMES STENNING.
Mr. JOHN ROWE DUTTON.

NOMINATIONS :—

(1) JAMES HANCOCK BRINKWORTH, Lecturer in Physics at St. Thomas' Hospital, 31, Albert Mansions, Battersea.
Proposed by Charles E. S. Phillips.
Seconded by Sidney Russ.

(2) FREDERICK JAMES HARLOW, B.Sc., St. Nicholas, Castlebar Park Road, Ealing, W.
Proposed by C. E. S. Phillips.
Seconded by R. Knox.

(3) J. STEPH. V. d. LINGEN, Lecturer S. A. College, Cape Town.
Proposed by J. H. Gardiner.
Seconded by R. Knox.

Mr. H. E. DONNITHORNE read a paper on " A New Modification of the Ionization Method of Measuring X-Rays."

Major WILSON showed " A New Tungsten Arc Lamp."

"A NEW MODIFICATION OF THE IONIZATION METHOD OF MEASURING X-RAYS."

By H. E. DONNITHORNE.

Most of the ordinary methods of estimating the output of an X-ray tube are unsatisfactory, and the *most* unsatisfactory—taking the milliampères through the tube—is the most usual. It takes no account of distance and it makes no allowance for the variation of quantity with hardness, though the quantity of rays per milliampère varies greatly with the hardness.

All operators know that a good deal of the energy supplied to the tube is wasted in the form of heat, but few realize how great the quantity is. The anode gets very hot, owing to the cathode ray bombardment, and the energy thus absorbed as heat can be measured in the case of a water-cooled tube by noting the rate at which a definite quantity of water in the cooling chamber gains heat. Dr. Coolidge has done this and found that 99 per cent. of the energy supplied to the tube is lost in this way alone. Thus at one fell swoop we are left with only 1 per cent., and out of this we have to allow for heating of the residual gases by collision, of the glass by stray bombardment and the serious loss due to electrical leakage along the glass walls of the tube, as well as other smaller losses.

Obviously then the proportion of energy actually converted into X-rays cannot be much more than one in a thousand, and is probably less, so that any slight change in efficiency may make a very large difference in the quantity of rays. An increase of $\frac{1}{10}$th per cent. in the efficiency might double the output, and this without showing any change in the milliampèrage.

Again, the only instrument available for ordinary use in measuring such small currents is the moving coil, and its indications are not true indications of the rate at which energy is being delivered to the tube. Increasing the current through the tube so that the reading on the milliampèremeter increases from one to ten does not mean that ten times the energy is being delivered to the tube. If the current delivered were continuous and unvarying, the indications would be correct and represent the exact rate at which energy was being delivered to the tube, but in the case of an intermittent current, such as that supplied by a coil, it may read anything, depending on what the wave form is.

Again, think what happens when reverse current is present. A moving coil instrument,

supplied with impulses first in one direction and then in another, will subtract the two and read the difference. Thus a reading of 7 may mean an X-radiation due to 7 m.a. simply, or equally well that due to 10 m.a. plus a reverse radiation due to 3. An indication of 7, photographic power 10, fogging power 3, and burning power 13.

When we turn to other ways of measurement, the only well-recognised methods are by chemical reactions, such as B.P.C. pastilles, deposition of silver, etc. It is doubtful how far these give true measures of energy, but at least they appear to measure what we generally want to measure—the effect of the rays on the skin or photographic plate ; but they are all clumsy and waste time, and, in fact, take so long that they are useless for photographic purposes.

Just before the war, a third method was introduced into Germany—the selenium cell quantimeter. This consisted of a selenium cell (the resistance of which changes with light, X-ray or ordinary), connected up with a delicate galvanometer, the indications of which, changing with the change of resistance, show the intensity of the radiation causing this change.

The cell consisted of a small plaque in which the sensitive element was embedded so as to be protected from injury and shielded from ordinary light, and this plaque was placed wherever it was required to find the intensity. The connection to the galvo. was by means of a length of flex. This method is undoubtedly superior to the before-mentioned ones. The instrument takes only about one second to come to its reading, the measurement is made at the required spot, and the indication is due to the direct action of the rays, and is therefore not dependent on the generating apparatus. The selenium is a little sluggish in its action, and gets fatigued, so that several different readings taken in quick succession may not be very concordant. The particular allotropic form of selenium is rather unstable and inclined to degenerate into one of the other two forms, and so the instrument may become unreliable with age. For the measurement of ordinary light, selenium cells have been found unreliable, and so must be viewed with distrust for X-ray work.

The only other method of any importance is the ionization method, and this has been adopted in all research work as the most reliable and accurate of all methods.

According to modern theory, a beam of X-rays does not directly produce those effects we generally attribute to it. The beam when passing through matter, solid, liquid or gaseous, is more or less absorbed, that is, it gives up its energy, or part of it, to the matter through which it passes. Matter cannot take up energy without its state being in some way altered thereby, otherwise we should have the distruction of energy, and the effect in this case is to cause to be shot out electrons or corpuscules, as Sir J. J. Thompson prefers to call them.

Now cathode rays are in fact nothing more than corpuscules in flight, so that X-rays striking any matter give rise to cathode rays. If these cathode rays are brought to an abrupt stop by striking some more or less impenetrable substance, they give rise to X-rays again, but if allowed to pass through a gas they gradually give up their energy to the gas, and by some rather mysterious mechanism make the gas conducting by producing in the gas positively and negatively electrified particles—ions.

The degree of ionization depends on the quantity of rays, but is independent of their hardness in air and most gases neither is it affected by the temperature, but only by the nature and quantity of the gas. Thus, if the gas whose ionization is to be measured is enclosed in a sealed chamber, the conductivity imparted to the gas is an exact measure of the quantity of rays entering the chamber.

Since the mechanism by which the X-rays act, in ionizing air, affecting a photographic plate or the skin of a patient is the same—by the emission of corpuscules—we might well expect the ionization method to be a good measure of these effects, and all experience goes to prove that this is so.

The great difficulty in ionization measurements has always been in the smallness of the currents to be measured, one ten millionth of a milliampère being a largish current in this work, and

this is what has prevented its use in practical X-ray work up to now.

One of the best laboratory forms of instrument consists of a small gold leaf electroscope enclosed in a box of aluminium foil, with a small window covered with celluloid through which the gold leaf can be observed.

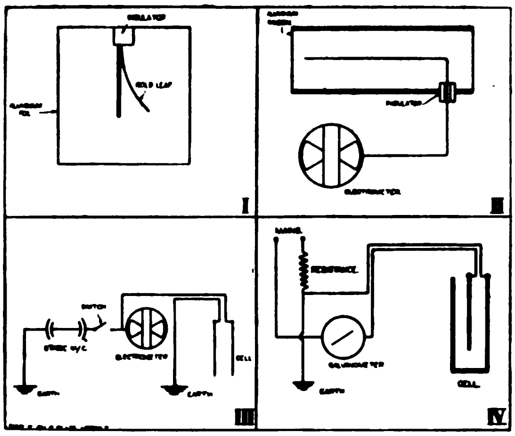


Diagram No. 1.

The electroscope is well insulated from the case and when the leaf is charged up it is repelled by the similarly charged plate, and remains sticking out at a considerable angle. If a beam of rays passes through the box it ionizes the air inside and the electricity leaks from the electroscope to the case, and as the charge diminishes the gold leaf gradually drops, the rate at which it falls measuring the ionization and thus the X-ray intensity. In practice the leaf is watched through a telescope and the time it takes to fall through a certain range noted.

It is often not convenient to use a combined ionization and electroscope chamber as described, and then a separate ionization chamber is used, made altogether of al. foil, or having an al. foil window through which the rays can pass, and in this is an insulated electrode connected by leads, which must be well shielded, to an electrometer. The walls of the chamber and shields of the leads are earthed to carry away any charge due to ions, and if the electrometer be charged up, the rate at which the reading falls indicates the leakage in the chamber due to ionization. Sometimes instead of earthing the chamber, it is connected to a source of high potential and ionization allows this potential gradually to charge up the electrometer.

Dr. Szillard has recently introduced an apparatus to measure X-ray dosage, which he calls an iontometer. By means of a small static machine he charges up an electrometer, which is then allowed to discharge through a small ionization chamber, the amount by which the reading drops indicating the dosage given. I believe the instrument has proved an accurate dosage meter, but static machines are an unpleasant complication, even when very small and simple, and electrometers are somewhat delicate.

Six months ago I proposed to Mr. Oschwald that we should try and apply the facts I have briefly outlined to the production of a simple instrument for the measurement of X-radiation in medical work.

Two things seemed necessary, an ionization chamber which would pass much larger currents than usual, and an indicating instrument, strong, reliable, sensitive and reasonably cheap. I think we have found both of these.

It is known that vapours of several liquids with low boiling points, such as ethyl bromide, gave much larger ionization currents than air, but unlike air they all seem more or less to differ with hard and soft rays, also they are mostly very active chemically, and therefore hard to confine, somewhat unstable and, being liquid, at ordinary temperatures and pressures one could never be sure of having exactly the same quantity of vapour in the chamber, which is, of course, imperative. We found, however, a method of treating the electrodes of the chamber which increased the ionization currents to a large extent and appeared quite independent of hardness in its action.

At the outset we had great trouble, owing to negative charges carried by the corpuscules, and found that the only safe form of cell consisted of an aluminium box, inside of which was placed an insulated aluminium sheet connected to the galvanometer by a well-insulated lead passing

into the box, and itself sheathed with a metal coating connected to earth. This, thoroughly carried out, kept us clear of troubles from electric charges and ionization of the air outside the chamber, and our results became concordant and satisfactory.

It is important that the whole cell should be included in the beam of rays. The cell should be placed where the photographic plate is to go, the current switched on and a reading taken, and from this the proper exposure can be estimated. Better still, the cell can be placed directly behind the plate, so that the rays pass through it after the plate and the exposure and reading made simultaneously. In long exposures, any change in the tube shows on the instrument, and can be allowed for. As the measurement of the rays is made after they have passed through the body to be photographed, all difficulty as to estimating the quantity absorbed by the body disappears.

Since the cell is fairly large in area, it measures the average intensity all over the plate, and so indicates that exposure which will give the best all-round result.

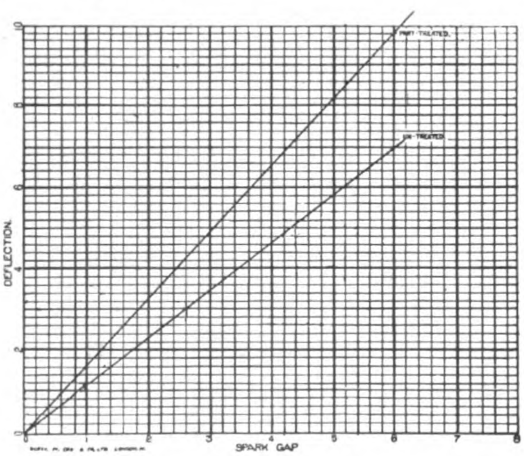


Diagram No. 2.

This slide shows the effect of altering the hardness of the tube, the milliampères being kept constant, and at first sight it seems to contradict what I said earlier as to the cell taking no notice of hardness. As a matter of fact, altering the equivalent spark length of the tube has two distinct effects. The mean penetrative power of the rays increases roughly as the square root of, and the quantity of radiation per milliampère directly as, the spark length.

This point is a little confusing and apt to be misunderstood, but is so important that I think it is worth a simple analogy to make clear.

I have here two ordinary metal filament lamps, each for 100 volts, but of different candle-powers, and I put them in series across the 200-volt mains.

Exactly the same current flows through each, as they are in series, but the small lamp is giving a great deal more light than its fellow, and the colour of the light is different—whiter ; that is, it is giving more light of a shorter wave length, but the more marked difference is in the quantity. All this corresponds exactly to the difference in action between two tubes of different hardness, but otherwise identical, with the same current flowing through each. The harder tube corresponds to the smaller lamp and gives much more rays of a shorter wave length.

	Over.	Normal.		Identical.		Under.	
			Best.				
Order of density	(1)	(2)	(3)	(4a)	(4b)	5	6
Current M.A.	1	8	1	12	4	8	12
M.A. seconds	72	162	47	242	72	72	72
Exposure according to intensimeter	648	422	422	422	422	189	126

III. Comparison of photographs of Benoist Radiometer.

I have here some photographs of a Benoist radiometer, chiefly remarkable for their badness, but they show fairly well that the intensimeter is a better guide than the milliampèremeter. The experiment also shows what Mr. Schall mentioned at the last meeting, that the radiation from the Coolidge tube is not directly proportional to the current, as shown on a moving coil instrument. With an ordinary tube, agreement seems better. The reason for the very long exposures is that the plates were slow ordinary ones, and the distance large. No. 3 was taken with a normal exposure, which we knew to be about right, and the reading of the intensimeter, multiplied by the number of

seconds exposure, gave a constant of 422, while the milliampère seconds were 47.

For all other currents two exposures were made, one giving an equal exposure according to the milliampèremeter—72 milliampère seconds, the other an equal exposure according to the intensimeter. It will be seen that the intensimeter was much the better guide.

M.C.READING	THERMO READING	INTENSIMETER
19	32	22
19	32·5	22
19	32·5	22
19	32·5	22
18	32·5	22
18	33	22
17	33·5	22
17	33·5	22
17	33·5	22
17	34·5	22
15	34·5	22
15	35·7	22
15	35·7	22
15	39	22

Diagram No. 4.

(iv.) TABLE SHOWING TUBE LEFT ON.

This table is interesting, as showing that reverse current can pass through a Coolidge tube and also as illustrating what I was saying about the effect of reverse current on a moving coil instrument. We had been getting discrepant readings and were led to conclude that they were due to reverse current, and so we placed in circuit in series with the moving coil milliampèremeter a Duddell Thermoammeter, an instrument which is somewhat akin to a hot wire ammeter, and reads correctly on any type of current. The two instruments we checked against each other, and there was no reverse when the current was switched on. The first readings show that the moving coil instrument is a very poor guide to the energy being supplied. For a little time everything kept steady, but soon the target got hot enough to give off ions on its own account, and reverse current began to pass; this rapidly increased till at last the change got so rapid that readings could not be taken, and the moving coil finally came to zero, the Duddell to about 80 or 90, and the intensimeter still about 22.

The moving coil was showing the difference between forward and reverse, the Duddell sums the two, and the intensimeter, being too far away to be affected by the soft scattered radiations, indicates the radiation due to the forward current only.

I wish to acknowledge my indebtedness to Mr. Oschwald, who has done quite as much of the work as myself, and to the South Western Polytechnic for allowing us the use of their electrical laboratory and instruments.

Major WILSON said: I want to add my little quota of thanks to Mr. Donnithorne for his excellent and painstaking paper. I differ a little from Mr. Schall with regard to the milliampèremeter. If the milliampèremeter would only tell us what the maximum reading really was, then it would be valuable. It does not tell us what we have in the tube when there is inverse current, and to say that it does is to credit it with more than its value. Mr. Donnithorne's instrument means using the under-couch tubestand, for otherwise I do not see how he could comfortably get a chamber of that size underneath the patient and under the plate. The amount of absorption due to the ordinary table itself is very material, and would be a cause of some fallacy in the readings. I should like also to protest against a little omission made by Mr. Donnithorne, perhaps involuntarily. One rather expects in a scientific society that when a statement is made as to an end obtained, the means by which that end is obtained should be given. He said that he had succeeded in getting some material for coating his chamber which had successfully made the air more conducting, and then he stopped. He did not tell us what the material was. Perhaps he will tell us later.

Mr. SCHALL said: I think we all have reason to be very grateful to Mr. Donnithorne for the paper he has read to us, because, admittedly, the measurement both of the quantity and quality of X-rays is quite the most pressing problem at the present time. It is really a pity

that we workers in X-ray matters should be without instruments accurately to measure the rays we are dealing with, whilst our colleagues in other branches of electrical engineering can measure their currents and so forth with absolute accuracy. There are two points I should like to raise in connection with the paper. The first is this: I think that any instrument that is going to measure X-rays must either absorb those rays completely, or else we must know exactly what percentage of the total radiation is being absorbed. If the radiation is being absorbed altogether, we know what the amount of the radiation was; again, if we know that 50 per cent. is being absorbed and the rest passed through, then we know that the total amount of radiation was double what we measured; if 25 per cent., then the total amount of radiation was four times what we measured. But if we have got an instrument with which we do not know exactly how much is being absorbed, we are considerably in the dark. Obviously the amount absorbed by any instrument varies with the penetrative powers of the rays. On that point I am not clear as to whether this intensimeter fulfils the condition or not.

I think also that Mr. Donnithorne was a little too hard on the milliampèremeter. He states that the meter does not measure the total value of the current passing through the tube. I agree; but I think it measures some kind of mean value between that total value of the current passing through the tube at each impulse and the time which elapses between two successive impulses. Supposing we have thirty impulses per second, and that at the highest point of the wave we have got a current of 2 m.a. flowing through the tube. The milliampèremeter will read, for instance, ½ m.a. If, now, we increase the number of impulses to sixty, the milliampèremeter will read 1 m.a. Now, if we have an instrument similar to the thermo instrument of Mr. Duddell, it will read the maximum value of the impulses in both cases, yet in the one case we are getting only half the energy we are getting in the other. Therefore, the milliampèremeter, while not giving the exact value, gives an arbitrary and relative value. I think that the milliampèremeter can certainly be relied on for ordinary radiography, provided account is taken of the penetrating power of the tube, the distance between the tube and the photographic plate, and so forth. It is for treatment, and particularly for deep therapy, that an instrument is required, and it is in connection with that kind of work that I have had great hopes of ionization methods. But I am a little doubtful of late, because it seems to me that the ionization instrument suffers from the same defects as every other method hitherto. We do not know what proportion of the radiation is absorbed by the measuring instrument, and therefore we cannot tell what is the total radiation.

Mr. DONNITHORNE said: In reply to the point raised by Mr. Schall, I think that the remarks of Major Wilson meet the question as to the readings of the milliampèremeter. Under modern conditions there is always a considerable amount of current passed through the tube in the wrong direction amounting in some cases to 15 or even 20 per cent. of the total.

When one forces an induction coil, the inverse current runs up at a very much higher rate than the direct. One does not gain much, but one gets a higher potential on "make." Generally one puts in two valve tubes in series to try and get rid of this inverse current. In any case like that, a moving coil milliampèremeter is not trustworthy. What Mr. Schall says about the number of impulses is right, but against that, the instrument makes very little allowance for change of wave form. Two waves of quite different form may read exactly the same on a moving coil milliampèremeter. With this instrument there was failure to discriminate between the rays due to low potential, seen after break, before the potential had time to rise to anything like its full value, and the portion of the wave producing the useful ray. Major Wilson mentioned the position of the instrument in relation to the couch. Where the instrument cannot be kept under the plate, allowance must be made. The wood of the couch does not stop very much. As to the question of the material employed for coating the chamber, it was not an unintentional

omission on my part. I have applied for a patent for it, and naturally, until the patent is granted, if it is worth it, I cannot reveal its nature. Afterwards, of course, I shall have no objection whatever to making it known.

A NEW TUNGSTEN ARC LAMP.

By Major WILSON.

I have merely to demonstrate the little lamp I spoke of at the last meeting of the Society. It is a semi-enclosed tungsten arc for treatment by means of ultra violet radiations. My idea originally was simply to produce a lamp capable of giving us an ultra violet ray, rich in the shortest wave lengths, but much more cheaply constructed than the ordinary large arc in use at some of our hospitals, and at the same time more effective than the ordinary Finsen-Reyn lamp, and costing less for running than the Simpson arc. Tungsten vapour giving a very short ultra-violet ray, it seemed simple and obvious to get a tungsten vapour and burn the ordinary arc, but I found in the course of experiments that if we had two movable carbons, as we usually had, the arc was never so steady as it was when one of them was immovable and the other was moving. Therefore the lower portion was made immovable and of pure tungsten, while the top portion was movable and made of carbon. This top portion is simply carbon cored with tungsten, the tungsten powder being made into paste and forced into the ordinary carbon, forming the filling of the central cavity, and dried. When the arc is formed, the tungsten is reduced in the intense heat generated, and becomes metallic tungsten. We tried several other materials, including cerium, but the tungsten was about as good as any, and that is the arrangement I propose using. We also tried carbon to carbon, tungsten block to plain carbon, carbons cored with tungsten, etc., but the carbon cored with tungsten powder was most successful. I have some slides which were kindly made for me by Mr. Merton, of the London University, showing the results obtained spectroscopically with this light. We get down to a wave-length of 2,000 Ångström units. There is no sputter, as in the case of radiations from other lamps. The arc works quite automatically. The current consumption is remarkably low, from 2½ to 4½ ampères. I have several diaphragms for use in front of it in order to narrow the beam, but for treatment purposes I employ a cylinder, for the reason that in using ultra-violet light it is important that the light should be cold, and a quartz chamber pressed against the skin and enclosing circulating water answers the double purpose of cooling the ray and expressing the blood from the part under treatment, and this has advantages over the usual method. A good quartz compressor, air-cooled, would, I think, be more effective than the water-cooled.

Dr. E. P. CUMBERBATCH said that the results which he personally had obtained after ten months' experience with the tungsten arc light showed that the tungsten arc lamp had come to stay, and Major Wilson's invention conferred a benefit by introducing a lamp, the cost of the electrodes of which was very much less than that of the Simpson lamp they were using just at present. The amount of vapour was quite small, and that was an advantage when the lamp was used in a consulting room, because, with the Simpson arc lamp used under such circumstances, within an hour or two every object in the room would be covered with a white sediment which was tedious to remove. He would like to ask what form of mirror Major Wilson used. Was it of plain metal or metal covered with quartz, and was it movable? An improvement would be to arrange that the focus of the mirror should be at the poles, so as to bring the reflected rays back to the poles. Still further, one might have a movable quartz lens on the front of the lamp, so that the rays given out were directly brought to a focus, made parallel or divergent.

The PRESIDENT: With regard to the transparency of quartz to ultra-violet radiation, next to fluorite it is the most transparent of all substances. Has Major Wilson tried uranium oxide as a core to his carbon rods? The uranium radiations extend a very long way into the ultra-violet.

Major WILSON said in reply to Dr. Cumberbatch, that the mirror he used was an ordinary mirror. His idea was to have a speculum mirror of solid metal—a very heavy metal mirror. The curvature of the mirror was such as to give a very slightly divergent ray. It was focussed to give that at an average distance of 12 in. from the light source. As to Dr. Cumberbatch's suggestion with regard to the quartz lens, he had purposely avoided using anything between the arc and the substance to be treated. He did not know up to what wave-length quartz would absorb.

NOTES.

AFFILIATION WITH THE ROYAL PHOTOGRAPHIC SOCIETY.

As a result of the affiliation announced in April, "The Red Book of the R.P.S." is enclosed with this issue to all members residing in London and the suburbs; other members can otbain them by sending a post-card to Dr. Knox.

The "Red Book" provides for one free admission to the R.P.S. Exhibition on Red Book Evening, Friday, August 25th, 6 p.m., and also includes a number of half-price tickets which can be used at any time during the period of the Exhibition, Aug. 21st—Sept. 30th.

It is expected that there will be an interesting exhibition of Radiographs, and it is to be hoped that many of the Röntgen Society members will not only send work for exhibition (entries to be made on forms to be obtained from the Secretary, R.P.S., 35, Russell Square, W.C., close on July 28th), but be present on this particular evening when the Exhibition is thrown open to members of affiliated societies on production of the current copy of the "Red Book."

X-RAYS AND THE THEORY OF RADIATION.

THE Bakerian Lecture at the Royal Society on May 25th was delivered by Professor C. G. Barkla, F.R.S. The three forms of emission, scattered X radiation, Fluorescent (Characteristic) radiation, and Primary X-rays, were considered in detail.

SCATTERED RADIATION is shown to be a radiation emitted by electrons whose motion is controlled by the primary radiation and recent experiments have shown that scattering takes place by groups of electrons rather than by individual electrons when the wave-length is comparable to the size of a group.

FLUORESCENT (CHARACTERISTIC X-RADIATION) is uncontrolled by the primary radiation exciting it, the emission is conditional upon the primary beam being of shorter wave-length (an extension of Stokes' Law). PRIMARY X-RADIATION.—It is probable that the process of generation of X-rays is one of the same character as that resulting in the emission of characteristic radiation, there is no conclusive evidence of a limit to the penetrating power which may be generated at any particular anti-cathode.

ROYAL INSTITUTION.

On the same evening Professor Barkla lectured upon the same subject at the Royal Institution, there was a large audience at which many well-known authorities upon X-rays were present. In the library of the Institution after the lecture many exhibits of interest were shown and a short demonstration was given upon OCCURRENCE OF URANMITE, illustrated by radiographs of Pitchblend from various localities, by Mr. J. H. Gardener.

TRANSFORMATION OF HELIUM AND NEON.

IN the annual report of the progress of Chemistry, reference is made to the experiment of Collie and Patterson that suggested the transformation of helium and neon by passage of the electric discharge through pure hydrogen. Further investigations by A. C. C. Egerton, R. J. Strutt, and T. R. Merton, gave negative results, and the conclusion is reached that "There appear to be no adequate grounds to warrant the interpretation of the results obtained by Collie and Paterson in favour of the theory of transmutation."

INTERNATIONAL ATOMIC WEIGHTS.

THE International Committee of Atomic Weights has adopted changes in the values of twelve elements as follows:—

Helium	4·00
Radium	226·0
Ytterbium	173·5
Carbon	2·005
Lead	207·20
Praseodymium	140·92
Sulphur	32·06
Uranium	238·2
Tin	118·7
Lutecium	175·0
Yttrium	88·7
Molybdenum	96·029
Columbium	93·13

HERITAGE CRAFT SCHOOLS, CHAILEY, SUSSEX.

WE have been asked to give publicity to a work that has been started in aid of the many sorely crippled soldiers who are the unfortunate first fruits of the present terrible war. Men discharged from our hospitals minus a limb or limbs, or otherwise permanently disabled, are faced with a very sad future, unless some means can be devised by which their minds can be kept occupied, and in spite of misfortune that in some instances would appear to render their case hopeless, they may be trained to do useful work.

The association with young people, and instruction into the working of the numerous crafts practised at Chailey, is a novel, and has so far proved to be a highly successful experiment, and has produced the happiest results.

In the list of Patrons and Executive Council are seen names familiar to many of us.

Naturally the extension of the interests of the institution to cope with the present need necessitates a very large expenditure, and any help that can be rendered will be gratefully received.

The Hon. Secretary is Mrs. C. W. Kimmins, Chailey, Sussex, who will forward booklet giving further particulars to any interested.

ABSTRACTS.

The following are selected from the current numbers of " SCIENCE ABSTRACTS *" as likely to be of special interest to members of the Society, and are published by permission of the Editors of that Journal.*

331. *Life of Radium.* E. GLEDITSCH. (Amer. J. Sci. 41, pp. 112-124, Jan., 1916.)—The author has made a series of measurements of the growth of radium in solutions of ionium preparations separated from different uranium minerals—namely: (1) from uranite (North Carolina), (2) from clevite (Norway), and (3) from bröggerite (Norway). The results obtained from the first two minerals indicate that the constant of change of Ra has a higher value than the one generally accepted, $3 \cdot 48 \times 10^{-4}(\text{year})^{-1}$. Neither of them, however, can claim a very high degree of accuracy for reasons given in the paper. The objections raised in the case of uranite and clevite do not apply in the case of bröggerite—a mineral which is one of the oldest uranites, and particularly free from all alteration products.

The results obtained for the most carefully prepared solutions are:—(*a*) The disintegration constant of radium has a value of $4 \cdot 22 \times 10^{-4}(\text{year})^{-1}$, and $4 \cdot 14 \times 10^{-4}(\text{year})^{-1}$. (*b*) The half-value period of Ra indicated is accordingly 1642 years and 1674 years. The result agrees very closely with the value found by Rutherford [Abs. 180 (1915)]. A. B. W.

335. *Relation between Atomic Weights and Radioactive Constants.* F. G. CARRUTHERS. (Nature, 96, pp. 565-566, Jan. 20, 1916.)—Plotting the values of "log. range" and "log. atomic weight" for all the radio-elements emitting α-particles, it appears that each group of isotopes gives a straight line, and the lines for the various groups are approximately parallel and equally spaced. From these relations it is deduced that the range of the α-particle is universally proportional to about the 21st power of the atomic weight of the element emitting it. The groups, arranged in the fashion indicated above, follow one another in a rational order. The above relationships were detected as a result of noticing first the evident family resemblance between the γ-rays in the isotopic groups. Among the β-rays something of the same sort is noticeable, "μ" generally falling with increasing atomic weight, but for the very soft β-rays μ is directly proportional to the 33rd power of atomic weight. A. B. W.

376. *New X-ray Tube for Spectroscopic Work.* M. SIEGBAHN. (Deutsch. Phys. Gesell., Verh. 17. 24. pp. 469-470, Dec. 30, 1915.)—A description is given of a metal X-ray tube, the anode of which is insulated by a porcelain sheath. A peculiar cooling device is attached, and the antikathode (fixed directly on the end of the tube facing the anode) that is generally used with the tube, consists of a piece of silver-foil 0·15 mm. in thickness. This tube is particularly useful in the study of soft X-radiations, as well as in X-ray spectroscopy. A. B. W.

446. *Atomic Weight of "Uranium-Lead."* O. HÖNIGSCHMID and S. HOROVITZ. (Monatsh. d. Chemie, 36. pp. 355-380, May, 1915.)—Lead ores may be supposed to contain Pb, AcE (generally accompanying U, from UY, atomic weight probably 210), and RaG or uranium lead (from RaF or polonium by loss of one α-ray). The minerals examined are: (1) purest pitchblende from Joachimsthal; 20 kg. of this yielded a lead apparently of atomic weight 206·405, probably a mixture of RaG, AcE, and ordinary lead; (2) a crystallized uranium ore from Morogoro (German East Africa, primary geological formation), studied by Marckwald, gave a lead of atomic weight 206·046, and was probably pure RaG, the last decay product of uranium; (3) bröggerite from Moos, Norway (also very old formation), free from heavy metals, containing 79% U_3O_8, 4·5 ThO_2, 9·5 PbO, yielded a lead of atomic weight 206·063, like (2), therefore pure RaG. In addition a determination (4) was made of the atomic weight of common lead yielding 207·18 in agreement with T. W. Richards and Lembert, and with Baxter, Wilson and Grover. Quartz vessels were used for distillations. The arc and spectra of (2) and (4) (reproduced in the paper) proved identical.

The conclusion is that RaG is really an isotope of lead. H. B.

566. *Röntgen-ray Spectra.* A. W. HULL. (Frank. Inst., J. 181. pp. 423-424, March, 1916.)—This is an account of an attempt to measure accurately the quality and intensity of X-rays from a Coolidge tube. In the case of a tungsten antikathode, it is observed that there is a very rapid decrease in the wave-length, and at the same time an increase in intensity for all wave-lengths, if the voltage is increased. A penetrating ray free from soft radiation can be obtained only by using a filter exhibiting selective absorption for the latter. Measurements of the spectrum at 70,000 volts with and without an Al filter of 3 mm. thickness, indicate that the intensity of the soft rays has been reduced much more than that of the penetrating rays, but there is still a good deal of the former left. By using thicker filters to reduce still further the intensity of the soft rays, the intensity of the penetrating rays would also be cut down to a small fraction of their initial intensity, but with a powerful tube, such as is in process of development, it is hoped that there may still be left sufficient rays for practical work. [See also Rutherford, Barnes, and Richardson, Abs. 1417 (1915).] E. A. O.

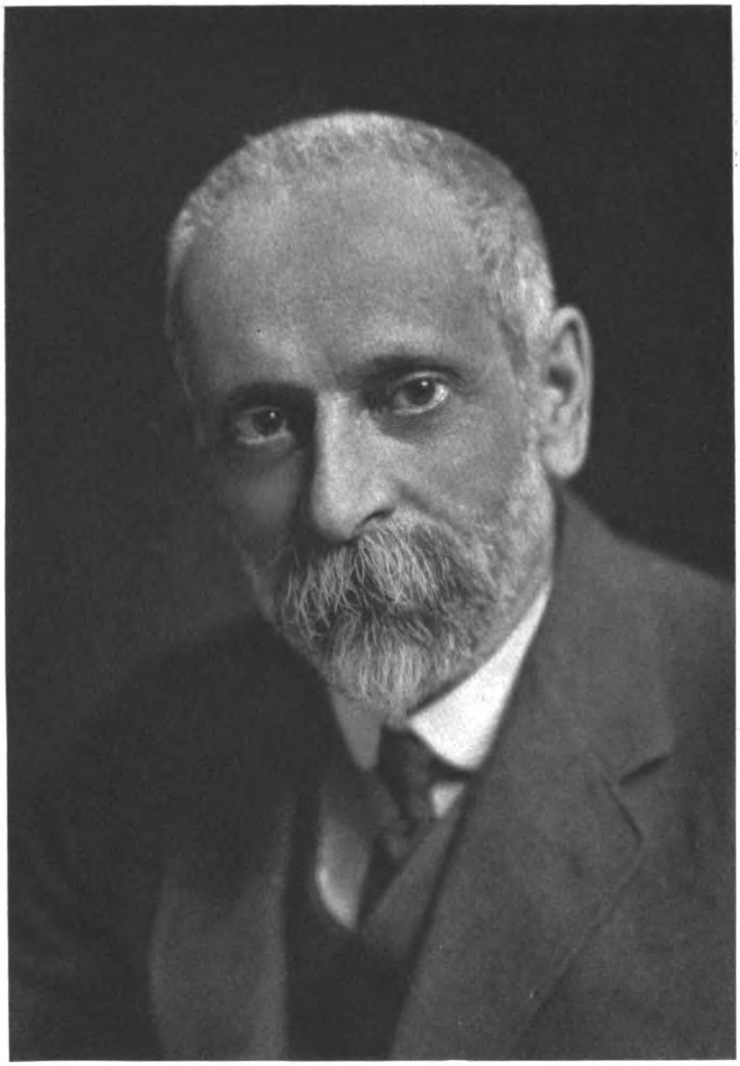
567. *X-ray Spectrum of Tungsten.* W. S. GORTON. (Phys. Rev. 7. pp. 203-208, Feb., 1916.)—In the work on the X-ray spectra of the elements, tungsten has been used as frequently as any other element, with the possible exception of platinum, but results of various observers are not in good agreement. In evidence of this the work of de Broglie, Herweg, Moseley, W. H. Bragg, etc. [see Abs. 243, 244, 675, 877 (1914)], with this element is reviewed briefly. In the present investigation a Coolidge tube was used as the source of X-rays, and the tube was excited by a 10-k.w. interrupterless generator. This machine consisted of a step-up transformer and a synchronously revolving switch which rectified the high-tension e.m.f.

The "stationary crystal" method was employed, rock-salt and calcite being the crystals selected for the experiments. Tables of wave-lengths of lines shown in photographic reproductions are given, and the values compared with those of other observers. It was found that the results agree well with those of de Broglie (*loc. cit.*), but are uniformly lower, by 1 to 2·3%, than those of Barnes [Abs. 1420 (1915).] The cause of these discrepancies is discussed. A. B. W.

606. *The Operation of Induction Coils with Electrolytic Interrupters.* O. M. CORBINO and G. C. TRABACCHI. (Elettrotecnia, 3. pp. 42-46, Jan. 25,

1916.)—The chief difficulty in feeding induction coils with alternating current and electrolytic interrupters consists in the strong inverse secondary current generated when the primary current is negative in direction. The authors have therefore devised an electrolytic interrupter which at the same time acts as a valve, so as to avoid the establishment of the negative primary current. The new electrolytic interrupter is formed of a Pt or Fe point, an Al plate, and a solution of a salt capable of producing the known valve action with Al. The best liquid is either a 20% solution of sodium potassium tartrate or a saturated solution of sodium bicarbonate. This interrupter-valve does not require specially forming, but it forms itself in half a minute's working. The authors have compared the working of an induction coil fed by direct current through an ordinary electrolytic interrupter and the working of the same coil fed by alternating current through the new valve interrupter. Several diagrams were taken by using the Braun tube. The d.c. feeding produces a considerable inverse e.m.f. which follows immediately on the direct secondary current, and, finding, therefore, the X-ray tube in the excited condition, gives rise to an inverse current, which can be prevented only by the use of a secondary valve. The a.c. supply through the new valve-interrupter can, in favourable conditions, produce no inverse secondary current, so that no valve is required in the circuit of the tube. This is due to the fact that the inverse e.m.f. is smaller than in the former case, and is produced a long time after the direct current has ceased: the tube being no longer in the excited state does not allow the inverse current to pass. The favourable conditions for obtaining this result consist in giving a suitable value to the self-induction of the primary and in letting the small electrode of the interrupter dip far enough in the liquid. As a matter of fact, whiat is called "direct current" is really in all cases a damped alternating current due to the self-induction and capacity of the secondary of the coil; but the inverse currents due to these rapid oscillations have no bad effect upon the X-ray tube. E. B.

616. *Radiography of Metals.* W. P. DAVEY. (Am. Electrochem. Soc., Trans. 28. pp. 407-418, 1915.)—The results of the examination of a steel casting [Abs. 172B (1915)] were so promising that it appeared desirable to study the general technique of the radiography of metals. Formulæ have been worked out for the calculation of the exposure required for steel plates of various thicknesses, using 15-in., 13-in. and 11-in. spark-gaps. While the effective penetration at a 13-in. gap is greater than at an 11-in. gap, no greater penetration is obtained by the use of a 15-in. gap, but there is a marked decrease in the exposure required consequent upon the increase in the voltage across the tube. At present it is impracticable to radiograph through more than 1½ in. of steel, and, commercially, radiography is only possible up to a thickness of ½ in. The saving of time gained by the use of a 15-in. spark-gap as against a 13-in. gap, however, points to the fact that a further increase of the voltage across the tube would make the radiographing of still greater thicknesses of steel a commercial possibility. Published data on the scattering of X-rays in Al lead to the expectation that the use of high voltages would result on the production of blurred images, but this does not appear to be the case. Experiments show that by the use of a 15-in. spark-gap it is possible to detect an air inclusion 0·021 in. thick in 1¼ in. of steel, and an air inclusion 0·007 in. thick in ⅜ in. of steel. F. C. A. H. L.



James H. Gardiner, F.C.S.
President of the Röntgen Society, 1915-1916-

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RÖNTGEN SOCIETY.

THE ANNUAL GENERAL MEETING of the Society was held at the Institution of Electrical Engineers on Tuesday, June 6th, 1916, the President, Mr. J. H. Gardiner, F.C.S., in the chair.

The minutes of the last annual meeting and of the previous ordinary meeting were read and confirmed.

The following were unanimously elected members of the Society :—

Mr. J. H. BRINKWORTH.
Mr. F. H. HARLOW, B.Sc.
Mr. J. S. V. D. LINGEN.

NOMINATIONS.

H. C. SHANNON.

Proposed by SILVANUS P. THOMPSON.
Seconded by B. H. MORPHY.

A. RUSSELL GREEN, M.B., B.A.

Proposed by ROBERT KNOX.
Seconded by GEORGE H. RODMAN.

The President said that it had been decided that as the Annual Report was a particularly important document this year, a small subcommittee should be appointed to consider it, and it would be published after their consideration in the next issue of the Journal.

With regard to the election of President, Officers and Members of Council, as there had been no further nominations he took it that the names published and circulated to serve for the session 1916-1917, were accepted (agreed unanimously).

Past Presidents :

1897-8. Prof. SILVANUS P. THOMPSON, F.R.S. (The Late).
1898-9. C. W. MANSELL-MOULLIN, M.A., M.D., F.R.C.S.
1899-0. WILSON NOBLE, M.A.
1900-1. J. MACINTYRE, M.B., C.M.
1901-2. HERBERT JACKSON, F.C.S.
1902-3. HERBERT JACKSON, F.C.S.
1903-4. Lord BLYTHSWOOD (*The Late*).
1904-5. C. THURSTAN HOLLAND, M.R.C.S., L.R.C.P.
1905-6. F. SODDY, M.A., F.R.S.
1906-7. C. V. BOYS, F.R.S.
1907-8. WILLIAM DUDDELL, F.R.S.
1908-9. W. DEANE BUTCHER, M.R.C.S.
1909-10. C. E. S. PHILLIPS, F.R.S.E.
1910-11. G. H. RODMAN, M.D.
1911-12. A. A. CAMPBELL SWINTON, M.I.C.E., M.I.E.E.
1912-13. Sir JAMES MACKENZIE DAVIDSON, M.B.
1913-14. Prof. A. W. PORTER, F.R.S.
1914-15. Sir ALFRED PEARCE GOULD, K.C.V.O.
1915-16. J. H. GARDINER, F.C.S.

Honorary Members :

Sir W. CROOKES, O.M., F.R.S.
Prof. Sir OLIVER LODGE, F.R.S.
Prof. Sir E. RUTHERFORD, D.Sc., F.R.S.
Prof. STEPHAN LEDUC.
A. H. PIRIE, M.D.
Prof. RÖNTGEN.
Prof. GUIDO HOLZKNECHT.

Honorary Corresponding Member :
Dr. JEAN CLUNET, Paris.

Mr. G. PEARCE, Hon. Treasurer, presented the report of the financial condition of the Society.

Annual Report of the Council.

1915-1916.

THE Council has pleasure in recording another successful session. The attendance at the meetings has, under the existing circumstances, been remarkably good, though there is still room for larger numbers.

During the session many applications have been made for membership and of these Council have nominated twenty-four, who have been duly elected members of the Society. There have been six resignations and three deaths, so the session shows a healthy increase of membership, and the Society has reason to be satisfied with the growing interest taken in its work. That so many members are at the present time intimately associated with the war may to some extent account for this satisfactory condition.

Perhaps a more powerful and decidedly more satisfactory explanation is the wider interest which the Society has been instrumental in promoting. Great impetus has been given to the study of radiology in several directions by the universal adoption of radiation in therapeutics throughout the country, the need for more powerful X-ray generators, tubes and accessories has had a stimulating effect upon the members of the Society, and given publicity to its existence.

There is still plenty of new ground to cover, many interesting fields of research yet await exploration and it should be the chief aim of the Society to encourage research in all possible ways.

The widely increasing fields of interest over which the Society exercises its influence may possibly *sooner or later* lead to an alteration in the name of the Society. Originally started as the Röntgen Society, the name then covered nearly all the subject matter—now when so many new and equally important subjects are dealt with it would appear that the need exists for a title which would be comprehensive enough to cover all.

In these days when destinies of societies, and even nations are, so to speak, in the melting pot, it would be well for the members of the Röntgen Society to realize that they can, in a small way, do something to aid in the rearrangement of things which will be essential after the

war is over. Steps should now be taken to consolidate the position of the Society so that its activities may not be confined to this country, but should be extended throughout the world; special steps should be taken to encourage free intercourse between ourselves and our allies; we, as a Society, should see that in the future these interests should not be allowed to drift apart.

Special efforts should therefore be made to promote the growth of these associations by increasing the number of honorary and corresponding members in our own and allied countries by the exchange of the Journal with all kindred societies; the general publicity which these means would give should lead to an increase in our membership list.

The Council would take this opportunity of pointing out to the designers and manufacturers of X-ray electro-medical and other therapeutic apparatus that a special effort should be made at the present time to produce in this country all kinds of apparatus used in X-ray work, so that when peace comes we should be prepared to take a leading place in the world's markets, a position which this country should always occupy.

The session was opened in November by a general meeting. The President read his address, which was full of interest and instruction; he gave a résumé of the scientific steps which had led to the knowledge of the present day and delighted his audience by many interesting side lights in the progress of science as seen from within. He particularly emphasized the point that it is the willing worker who is most desired in science, the man who works for work's sake and who throws his whole soul into it on that account.

It is a matter of regret that owing to the war the Committee appointed to deal with the Dosage question has not been able to carry on any work, but it is hoped that when times are more tranquil the Committee may resume its labours.

The Council has much pleasure in thanking the Institution of Electrical Engineers for the excellent accommodation provided for the meetings and the Officers of the Institution, who have always been so willing to make the arrangements for our meetings; this has in no small measure contributed to the success of the monthly meetings.

The December meeting was held at the Institution of Electrical Engineers, when Dr. Leonard A. Levy read a paper on "Some Remarks on Fluorescent and Intensifying Screens"; this was illustrated by several interesting experiments. Dr. Levy gave a most interesting description of the processes employed in the manufacture of these screens, and in addition gave a good deal of instruction on how to handle the screens in order to obtain good results; an interesting discussion was the outcome of the paper.

Mr. G. G. Blake read a short paper on "Further Notes on Localization." This was an addition to the contribution Mr. Blake made to the discussion at the meeting on "Localization of Foreign Bodies" last session.

The meeting held on January 4th was full of interest and was obviously the kind of meeting which finds favour with the members. The President opened by a short description, illustrated by lantern slides and plates, on "Some Observations upon the Occurrence of Uraninite (Pitchblende)."

Mr. P. J. Neate described and demonstrated a working model of a new therapeutic tube stand.

Mr. Charles A. Schunk showed a number of lantern slides.

Dr. N. S. Finzi and Dr. Eccles showed new screen rests.

Dr. Harvey described a Casette designed for the accurate localization of foreign bodies.

A Dubilier X-ray apparatus was exhibited by the General Electric Company.

The February and March meetings were devoted to a discussion opened by Dr. Sidney Russ on "The Injurious Effects Produced by X-rays." Sixteen members took part in the discussion, and Mr. A. C. Gunstone read a paper on "The Use of Inverse Current."

The April meeting was devoted to the consideration of "A Chronograph constructed to work with the Electroscope," exhibited by Mr. P. J. Neate.

Messrs. B. H. Morphy and S. R. Mullard presented a paper on "The Enclosed Tungsten Arc as a source of Ultra Violet Light." This was a most interesting paper and led to a considerable discussion.

Mr. E. Schall, B.Sc., read a paper on "Experiments with a Coolidge Tube." The facts demonstrated by Mr. Schall were of great interest and gave indications that in the Coolidge tube and its manipulation there is a wide field for investigation and future discussion.

At this meeting the Resolution and Recommendation drawn up by the Council, as the outcome of the discussion on injurious effects produced by X-rays, was formally put to the meeting.

The resolution and memorandum were submitted to the Admiralty and War Office and elicited letters from each authority which specifically stated that all possible precautions were carried out and that all installations were in the hands of experienced radiologists.

The May meeting was devoted to a paper by Mr. H. E. Donnithorne on "A New Modification of the Ionization Method of Measuring X-rays." The paper was largely a description of a new apparatus and was discussed by several members.

Major Wilson exhibited and described a new Tungsten Arc Lamp suitable for therapeutic use.

The Annual General Meeting was held on June 6th, when in addition to the usual business Professor J. W. Nicholson, M.A., D.Sc., read a paper on "Homogeneity of Visible Radiation," in which many important problems connected with the physics of the X-ray were discussed.

There was also a large exhibition of X-ray and electro-therapeutic apparatus. Had it not been for the difficulties connected with the war, shortage of labour and stress of work, this exhibition evening would have assumed much larger proportions, and it is hoped that in the near future an exhibition may be arranged that shall be thoroughly representative of the labours of the Society and to which the public may have full access, so that the growth and importance of radiology may be more clearly understood than it is at present.

THE JOURNAL.—Mainly on account of the increased expense and scarcity of labour in London the publication of our Journal has been placed in the hands of Messrs. Percy Lund, Humphries & Co., Ltd., of Bradford. It is hoped that this new arrangement will materially reduce the cost and at the same time maintain the high character of the publication. The Editor wishes to record his appreciation of the thoroughness with which the work has hitherto been carried out by the late publishers, Messrs. Smith & Ebbs, Ltd., and regrets the circum-

stances wholly connected with cost of production that have rendered the change, in the interests of the Society, necessary.

The Library has been enlarged by the addition of many books of interest and value, and thanks are due to the Librarian of the Institution of the Electrical Engineers for the courtesy shown in dealing with enquiries connected with the Society's volumes.

The acceptance of the Presidency by Captain C. Thurstan Holland for the coming session is in itself a guarantee that the interests of the Society will be still further fostered and extended, and the Council can only express the hope that under his guidance the increasing popularity of these meetings will continue. It is peculiarly fitting that at this time the Presidency should be in the hands of one who has done so much to place British Radiography on a high level and whose reputation in this branch of our work is so world-wide.

The appointment of Dr. Sidney Russ to the post of joint Honorary Secretary will help to further strengthen the bonds which link together the various elements of our membership.

The Council also appreciate the continuance in office of the Honorary Treasurer, who already in a short time has done so much towards putting the financial position of the Society on a sound business footing.

The election of C. R. C. Lyster, C. Howard Head and F. W. Higgins to the Council is another step towards the consolidation of the various elements in the membership.

The Council in looking forward to the coming session is fully aware of the strenuous times that may have to be passed through, but in view of the record of the last two years it can confidently look forward to a useful term of work and it is hoped members will combine to make it one of real progress towards an assured and prosperous future.

HONORARY TREASURER'S REPORT.

May 31st, 1916.

I HAVE pleasure in submitting to the Members of the Society, the Accounts for the year ended 30th April, 1916, and it will be observed that the surplus of Income over Expenditure for the period under review amounts to £13 0s. 2d.

This result may be regarded as satisfactory, as, although the income of the Society is larger than that of the previous year, the charges to expenditure are heavier by reason of the modified form in which the accounts are presented to you, the increased cost of printing and stationery, special expenditure in connection with issuing Recommendations for use of X-ray Operators, and a drastic writing down of the values previously attached to the stock of Journals and stationery.

In addition to this, the work of those responsible for conducting the operations of the Society was heavier during the year in question for various causes, and the extra expense incurred thereby is reflected throughout the accounts.

The Accumulated Fund of the Society now amounts to the substantial sum of £321 12s. 7d., and of this £270 is represented by liquid assets, the balance consisting of the Library, Instruments, etc., for which conservative values are shown on the Balance Sheet.

GEOFFREY PEARCE.

THE RONTGEN SOCIETY.

INCOME AND EXPENDITURE ACCOUNT FOR THE YEAR ENDED 30TH APRIL, 1916.

EXPENDITURE.	£ s. d.	£ s. d.	INCOME.	£ s. d.	£ s. d.
To Publication and distribution of Journals...	204 12 4		By Membership Subscriptions— For year ended 30th April, 1916	206 17 0	
,, Printing and Stationery, including Recommendations for Protection of X-Ray Operators	53 11 3		Old amounts collected during year	15 15 0	
,, Expenses of Meetings ...	19 12 10				222 12 0
,, Reporting	7 14 6		,, Advertisements in the Journal— For year ended 30th April, 1916	97 5 9	
,, Science Abstracts	5 0 6		Old amounts collected during the year... ...	18 14 0	
,, Sundry Expenses, including Postages, Clerical Assistance, etc.	24 9 0			115 19 9	
,, Portraits of Presidents— Amount written off ...	10 0 0		Deduct Agent's Commission (5 quarters)	30 14 0	
		325 0 5			85 5 9
,, Balance, being Surplus of Income over Expenditure for the year ended 30th April, 1916, carried to Balance Sheet		13 0 2	,, Sales of Journal		25 0 10
			,, Income from £200 India 3% Stock (less tax)		5 2 0
		£338 0 7			£338 0 7

BALANCE SHEET, 30TH APRIL, 1916.

	£ s. d.	£ s. d.		£ s. d.	£ s. d.
To Sundry Creditors		22 9 10	By £200 India 3% Stock		139 0 0
,, Accumulated Fund—			,, Library		20 0 0
Balance at 30th April, 1915	308 12 5		,, Instruments and Apparatus		12 10 0
Add Excess of Income over Expenditure for the year ended 30th April, 1916 ...	13 0 2		,, Furniture		5 0 0
		321 12 7	,, Portraits of Presidents		15 5 0
			,, Stock of Journals, Stationery, etc. ...		10 0 0
			,, Sundry Debtors— Subscriptions	£16 16 0	
			Advertisements	45 3 3	
					61 19 3
			,, Cash at Bank		80 8 2
		£344 2 5			£344 2 5

I have examined the above Balance Sheet, dated 30th April, 1916, and certify that it exhibits a true and correct view of the state of the Society's affairs as shown by the Books of the Society. The Bankers have certified as to the correctness of the cash balance and the Bank of England has certified as to the investment in India 3% Stock.

20, *Limes Avenue,*
Woodside Park, N. Finchley,
31*st May,* 1916.

(Signed) E. CARRUTHERS WEBB, *Auditor,*
Chartered Accountant.

LIST OF MEMBERS, JULY 1st, 1916.

Elected.	
3.12.03	Allen, W., M.B, C.M., 20, Sandyford Place, Glasgow.
4. 2.13	Andrews, Cuthbert, Esq., 47, Red Lion Street, Holborn, W.C.
	Ashwin, Miss Alice Maud, Waterloo Hill, Stratford-on-Avon.
1. 2.98	Baker, F. W. Watson, F.R.M.S., 313, High Holborn, W.C.
	Baker, T. Thorne, F.C.S., Fairleigh, Wareham Road, South Croydon.
7.11.11	Bailey, Charles Fred, M.D., 125, Marine Parade, Brighton.
3.12.12	Barclay, Alfred E., M.A., M.D., Kersal Bank, Kersal, Manchester.
	Barnard, J. E., Park View, Brondesbury Park, N.W.
1. 4.13	Barry, Thos. D. C., Lt-Col., I.M.S., Royal Society's Club, 63, St. James's Street, W.
	Bassett-Smith, P.W., M.D., Staff-Surgeon, R.N. Hospital, Haslar.
7.12.99	Batten, G. B., M.D, 2, Underhill Road, Lordship Lane, S.E (*C.*) (*Vice-President.*)
	Beach, A. C. G., 49, Park Road, West Dulwich.
5. 5.14	Berry, Dr. Martin, The Connaught Club, Marble Arch, W.
4.11.09	Bird, E. Beverley, L.R.C P., S.I., 15, Clarence Parade, Southsea, Hants.
3. 1.07	Blackburn, Thos. Low, M.D., 125, Western Rd., Port Elizabeth, Cape Colony.
5. 5.14	Blake, G. Gascoyne, 10, Onslow Road, Richmond.
2.12.13	Blackie, Alfred, M.A., 24, Alexandra Square, South Kensington, S.W.
7. 1.13	Booker, A. H., Esq., 37, Claremont Road, Highgate, N.
6.99	Boys, C. V., F.R.S. (Honorary), 66, Victoria Street, S.W. (*Past President.*)
7. 3.07	Brown, Percy, B.A., M.D., Harvard, 155, Newbury Street, Boston, Mass., U.S.A.
	Brooks, Dr. C. H., 2, Fitzjohn Avenue, Hampstead, N.W.
10. 5.98	Buhl, Major F. J., 44, St. Aubyns, Hove, Sussex.
2.12.09	Bullimore, W. R., 61, Clerkenwell Road, E.C.
7.12.05	Burnside, Evelyn E., Esq., 9, Clifton Road, Crouch End.
7.12.99	Butcher, W. Deane, M.R.C S., Holyrood, Cleveland Road, Ealing. (*Past President.*)
3.12.12	Bythell, W. J. Storey, B.A (Cantab), M.D., Beech Hill, Singleton Road, Kersal, Manchester.
3.98	Caldwell, E. W., M.D., 480, Park Avenue, New York City.
	Caldwell, J. R., M.B., C.H.B., Brooklands, Holmes Chapel, Cheshire.
	Caulfield, A. St. George, Vicars Hill, Lymington, Hants.
1. 2.06	Chambers, W. D. F., B.A., Cantab., 90, Gordon Road, Ealing.
3. 5.00	Chaplin, A., 49, Manor Road, Bexhill-on-Sea.
18. 4.01	Clark, Chas. Alex., L.D.S., 42, Welbeck Street, W. (*C.*)
	Clark, T., 96, Mildmay Road, N.
3. 3.96	Coldwell, W. A., 6, Mandeville Place, Manchester Square.
5. 3.03	Connor, F. Powell, F.R.C.S., I.M S, Resident Surgeon, Medical College, Calcutta.
	Cookson, F. Nesfield, M D. (Lond.) F.R.C.P., Eng., Taggscroft, Stafford.
97	Cotton, W., M.D., 231, Gloucester Road, Bishopston, Bristol.
17. 6.99	Crane, A. W., M.D., 420, Rose Street, Kalamazoo, Michigan, U.S.A.
97	Crookes, Sir W., O.M., F.R.S. (Honorary), 7, Kensington Park Gardens, Notting Hill, W.
	Cumberbatch, Elkin Percy, M.A., Oxon, B.M., M R.C.P., 15, Upper Wimpole Street, W.
3.12.12	Darling, Byron C., B.A., M.D., Harvard, 122, East 34th Street, New York City.
97	Davidson, Sir James Mackenzie, M.B., 26, Park Crescent, Portland Place, W. (*Past President*).
1. 6.11	Dean, Alfred, Leigh Place, Hatton Garden, E.C.
20. 4.15	Dineen, George P., "Royston," 108, London Road, Wembley.
6.12 00	Dodd, John, M.D., 14, King Street, New Walk Gate, Leicester.
20. 4.15	Donnithorne, H.E., 8, Crescent Road, Wimbledon.
5. 3.08	Doyle, A. A, F.R.C.S.I., Union Trustee Chambers, Brisbane, Queensland.
	Duddell, W., F.R.S. (Honorary), 56, Victoria Street, Westminster. (*Past President*).
	Dutton, John Rowe, Wintrath, Winscombe, R.S.O., Somerset.
	Eagar, Dr W. H., Capt. C.A.M.C., Moore Barracks Hospital, Shorncliffe.
	Eccles, Herbert Annesley, M.D.Lond., M.R.C.S., L.R.C.P., 97, Church Road, Upper Norwood.
2. 2.11	Ensor, George E, Assoc. I.E.E, Pretoria Dist. Hospital, South Africa, Box 201.
5. 1.11	Finzi, Samuel Neville, M.B., 107, Harley Street, W.
1. 6.15	Fleck, Alexander, B.Sc., University, Glasgow.
2.12.13	Forder, A. O, 55, Playfield Crescent, East Dulwich, S.E.
	Foss, Kenneth Mackenzie, 16, Richmond Bridge Mansions, East Twickenham.
3. 1.01	Foster, W. J., F.R.C.S., "Downs," 11, Bath Road, Reading.
5. 1.13	Fowler, Frank, M.D., 29, Poole Road, Bournemouth.
4. 6.12	Fowler, Wm. Hope, M.B., 21, Walker Street, Edinburgh.

Elected.	
11. 1.98	Frost, Edmund, M.D., "Chesterfield," Chesterfield Road, Eastbourne.
7.12.05	Fryett, A. G., F.R.M.S., Holmhurst, Beacon Road, Hither Green, S.E.
3. 7.02	Gamlen, H.E., M.B., B.S., D.P.H., Chadwick House, York Road, West Hartlepool.
97	Gardiner, J. H., F.C.S., 59, Wroughton Road, Balham, S.W. (*Editor of Journal.*) (*C.*) (*Past President.*)
	George, H. Trevelyan, M.A., M.R.C.S., 33, Ampthill Square, N.W.
97	Gifford, J. W., F.R.A.S., F.R.M.S., Chard, Somerset.
97	Glew, F.H., 156, Clapham Road, S.W. (*C.*)
6.10.99	His Highness Sir Bhagvat Singh, The Thakore Sahib of Gondal, c/o H. S. King & Co., 45, Pall Mall, S.W.
5. 5.14	Gould, Sir A. Pearce, K.C.V.O., 10, Queen Anne Street, W. (*Past President.*)
4. 3.13	Gunstone, Arthur C., 7, Poppleton Road, Leytonstone, N.E.
2. 6.10	Hall-Edwards, J., L.R.C.P., L.M.Edin., 103, Newhall Street, Birmingham. (*Vice-President.*)
1. 6.15	Hallam, Arthur Rupert, M.D., 305, Glossop Road, Sheffield.
2.12.09	Hampson, Wm., M.A., L.M.S.S.A., 8, West Chapel Street, Down Street, W.
7. 7.00	Hancock, William J., Weld Club, Perth, Western Australia.
5. 1.14	Hardman, Harwood Freak, M.D., 24, Hartfield Square, Eastbourne.
7.12.99	Harris, L. H. L., M.B., 215, Macquarie Street, Sydney, N.S.W.
5. 1.14	Harvey, John Owen, M.D. Lond., 33, Camden House Road, Kensington.
6.12.06	Hazleton, E. B., M.D., Carter Knowle, Abbeydale Road, Sheffield.
6. 6.07	Head, Howard C., 21, Bloxholm Road, West Norwood. (*C.*)
	Heath, F. H. R., 22, Abbotsbury Road, Weymouth.
7.11.11	Hernaman-Johnson, Francis, M.B., etc., 33, Cavendish Square, W.
2.12.13	Higgins, F. W., A.R.C.Sc., B.Sc., Somerset Lodge, The Avenue, St. Margaret's-on-Thames.
98	Holland, C. Thurstan, M.R.C.S., L.R.C.P., 43, Rodney Street, Liverpool. (*President.*)
7. 1.09	Holzhnecht, Prof. D. Guido (Honorary), Vienna, IX., Lagerethgaise 24.
7. 2.01	Hosking, W. H., Major, M.R.C.S., Masterton, N.Z.
7.11.11	Hugo, Dr. D. Del, M.B., C.M., Mansion House Chambers, Cape Town, S.A.
3.12.12	Humphries, Howard, M.D., 8, West Chapel Street, Mayfair, W.
3.12.12	Hutchinson, Donald H., M.D., St. Annes, Gordon Road, Lowestoft.

Elected.	
	Hutton, E. W., Guernsey.
1. 2.98	Ince, Francis, The Hermitage, Jarvis Brook, Sussex.
5.99	Jackson, Professor Herbert, F.C.S. (Honorary), King's College, Strand, W.C., and 49, Lansdowne Road, Holland Park Avenue. (*Past President.*)
6. 4.05	Jacob, F.H., M.D., M.R.C.P., 32, Regent Street, Nottingham.
7. 6.15	Jeffries, J. Finbar, 71, Esselen Road, Sunnyside, Pretoria.
17. 3.14	Johnston, Robert H. W., F.R.C.S.Edin., 25, King Street, Maidstone.
5. 5.14	Jolly, W., 45, Paddington Street, Baker Street, W.
	Jones, W. J., 6, St. Thomas Gardens, Havestock Hill, N.W.
7. 2.01	Joynt, R. L., M.D., F.R.C.S.I., 84, Harcourt Street, Dublin.
3.10.10	Judah, D., L.M.S., Bombay, M.B., B.S., London, Lansdowne House, Apollo Bunder, Fort No. 1, Bombay, India.
4. 2.13	Kaye, G. W. C., D.Sc., 76, Addison Gardens, Kensington, W.
3. 2.13	Kempster, C. R., M.R.C.S., Northolme, Upper Clapton, N.E.
10. 1.99	Kent, H. A., The Poplars, Maidstone Road, Bounds Green, N.
7.12.99	Killik, Arthur, Elveden, Esher. (*Vice-President.*)
	King, Francis Davis Owen, 77, Jesmond Avenue, Wembley Hill, Middlesex.
1. 6.15	Kinsman, W. E., M.P.S., St. James' Infirmary, Ousley Road, Balham, S.W.
3.12.12	Kirton, R. G., Fremantle, Western Australia.
5. 1.05	Knox, Robert, M.D., 7, Harley Street. (*Hon. Secretary.*)
3.12.12	Krohn, Hugo F., 1, Heath Close, Hampstead Way, Hendon.
	Laurence, C. E., 196, Great Portland Street, W.
3.12.03	Lawson, C. B., Major R.A.M.C., 2, Wellington Terrace, Sandgate, Kent.
4. 2.04	Lawson, David, M.A., M.D., Nordrach-on-Dee, Banchory, N.B.
	Ledoux-Lebard, Dr. R., 5, Rue des Ursulines, Tours, France.
	Leduc, Stephan, Prof. (Honorary), 5, Quai Fosse, Nantes.
3. 3.14	Leatham, Henry Blackburn, M.R.C.S., L.R.C.P., New Plymouth, New Zealand.
	Levy, Dr. Leonard A., M.A. (Cantab.), F.I.C., F.C.S., 26, Teignmouth Road, Cricklewood, N.W.
6.99	Lodge, Sir O. J., F.R.S. (Honorary), The University, Birmingham.
3.12.12	Prof. Dott. Ulrica de Luca, Docente nelle R. Universita di Roma, Piazza delle Terme, 47, Roma, Italy.
1.12.14	Lupton, Hartley, B.Sc., Royal Infirmary, Manchester.

Elected.

4. 2.13 Lyster, C R. Chaworth, M.R.C.S, 70, Wimpole Street, W.

97 Macintyre, J., M.B., 179, Bath Street, Glasgow. (*Past President.*)

3 1.01 Macleod, Neil, M.D., 406, Avenue, Joffre, Shanghai.

4. 6.12 McKendrick, A., F.R.C.S. Edin., 27, Chalmers Street, Edinburgh.

3.12.12 Mailer, Wm., M.B., C.M., 86, Alexandra Park Road, Muswell Hill, N.

97 Martin, William, M.B., West Villa, Akenside Terrace, Newcastle-on-Tyne.

Marsden, E., Victoria College, Wellington, New Zealand.

3.11.04 Martindale, W. Harrison, Ph.D, 10, New Cavendish Street, W.

Mason, J. W., 1, Crouch Road, Stonebridge Park, N.W.

12 Meerat, Indian General Hospital, c/o India Office, Whitehall, S.W.

3.12.12 Melville, Stanley, M.D., 10, Welbeck Street, W.

1. 6.15 Metcalfe, James, M.D, 123, Harley Street, W.

3.12.09 Meyrick-Jones. H. M., M.R.C.S., L.R.C.P., Ashfield, Bayshill, Cheltenham.

5. 5.10 Mijhre, Einar, A.M.I.E.E., 35, Oxford Drive, Waterloo, Liverpool.

10. 5.98 Miller, Leslie, 66, Hatton Garden, E.C.

5.12.07 Mond, Robert, The Elms, Avenue Road, Regent's Park; Combe Bank, Nr. Sevenoaks, Kent.

4.11.13 Morgan, J. Douglas, M.D., 2, Canadian General Hospitial, B.E.F., France.

5.12.11 Morison, J., M.D., 1, Jackson Lane, Highgate, N.

Morphy, B. H., Durlstone Manor, Denmark Hill, S.E.

7. 4.14 Morrison, J. M. Woodburn, M.B., B.Ch., Glasgow, Newellthorpe, Ashton-under-Lyne.

Morton, W. J., M.D, 19, East Twenty-eighth Street, New York, U.S.A.

97 Moulton, J. Fletcher, Lord Justice, K.C., M.P., F.R.S., 57, Onslow Square, S.W.

7.11.01 Myles, C. D., Major, R.A.M.C., M.B., B.Ch., c/o Messrs. Holt & Co., 3, Whitehall Place, S.W.

Neate, P. J., 49, Frognal, Hampstead, N.W.

3. 5.00 Newcastle, His Grace The Duke of, 11, Hill Street, Berkeley Square, W.

7. 4.14 Nicholson, J. W., M.A., D.Sc., Professor Mathematics, King's College, Strand. (*C*)

97 Noble, Wilson, M.A., Park Place, Henley-on-Thames. (*Past President.*)

4. 3.09 Notcutt, S. A., B.A., B.Sc., Constitution Hill, Ipswich.

6.12.06 Nutt, W. Harwood, M.D., 280, Western Bank, Sheffield.

97 Ogston, W., 18, Union Street, Inverness, N.B.

4. 1.06 Ollé, A. Kareema, Charlotte Street, Ashfield, Sydney.

Elected.

7. 1.13 Oram, Walter Charles, M.D., 43, Rodney Street, Liverpool.

2. 5.07 Orton, G. Harrison, M.A., M.D. Cantab., 67, Upper Berkeley Street, Portman Square, W.

2.12.13 Owen, E: A., B.A., M.Sc., 69, Teddington Park, Teddington.

Owen-King, F D., 77, Jesmond Avenue, Wembley Hill, Middlesex.

7.12.05 Pask, T. P., A.M.I.E.E., Electrical Dept., S. African Railway, Johannesburg, South Africa.

5. 1.05 Pearce, Geoffrey, Cotswold, Finchley Road, Golders Green, N.W. (*Hon. Treasurer.*)

97 Phillips, C. E. S., F.R.S.E., Castle House, Shooters Hill, Woolwich, S.E. (*Past President.*)

97 Philp, W. C., L.R.C.P., 19, North Claremount Street, Glasgow, W.

5.11.08 Pirie, Howard, M D., B.Sc., 3, Canadian General Hospital, B.E.F., France.

Porter, Prof. A. W., F.R.S., 87, Parliament Hill Mansions, Lissenden Gardens, N.W. (*Past President.*)

3. 3.14 Provis, Francis Lionel, M.R C.P., F.R.C.S. Edin., 51, Welbeck Street, W.

3. 3.14 Prowse, William Barrington F., M.R C.S., Hon. Radiographer Sussex Eye Hospital, 31, Vernon Terrace, Brighton.

5 12.07 Raffety, C. W, Fairlight, 12, Altyre Road, East Croydon.

Rao, Achyuta, Esq., Senior Surgeon with the Government of H.H. The Maharaja of Mysore Bangalore.

Rey, J. F., 9, Park Road, Bognor.

18. 4.01 Reynolds, Russell John, M.B., B.S. Lond., M.R.C.S, L R.C.P. Lond., Rossendale Lodge, 7, Streatham Hill, S.W.

4.12.02 Roberts, D. Prosser, 10, Campbell Road, West Croydon.

1. 2.00 Rodman, G. H, M.D., 4, Heath Mansions, Putney Heath, S.W. (*C.*) (*Past President*).

87 Röntgen, Prof. (Honorary), Munich, Germany.

6.1 :.00 Rose, Miss May, Children's Hospital for Hip Diseases, Sevenoaks.

7.11.01 Rosenberg, A., 259, High Holborn, W.C.

2. .12 Rowden, A. R., M.B., C.M., 5, Park Square, Leeds.

2.12.10 Russ, Sidney, D.Sc., Cancer Research Laboratory, Middlesex Hospital, W. (*Joint Hon. Secretary.*)

Rutherford, Ernest, Prof., F.R.S., D.Sc. (Honorary), University, Manchester.

2. 1.12 Salmond, R. W. A., M.D., 23, De Crespigny Park, Denmark Hill, S.E.

4. 1.06 Salomonson, Prof. D. Wertheim, The University, Amsterdam; 43, Vondelstrasse, Amsterdam.

3.11.13 Sayer, Miss Ettie, M.D., B.Sc.Lond., 35, Upper Brook Street, W.

3.01 Schall, K, 75, New Cavendish Street, W.

Elected.	
7. 5.12	Schall, W. E., B.Sc. Lond.
3. 3.14	Schunck, Charles A., "Ewelme," near Wallingford
2.12.10	Scott, Sebastian Gilbert, M.R.C.S., L.R.C.P., 6, Bentinck Street, W.
10. 5.98	Sharpe, Miss Margaret M., L.R.C.P. Edin., St. Bega, William Way, Letchworth.
3. 5.06	Shoolbred, Henry George, 15, Crescent Road, Luton, Beds.
20. 4.15	Simmons, Lucien Oliver, Sick Quarters, Royal Naval Barracks, Chatham.
2.12.13	Simpson, A. J. G., c/o Messrs. Siemens, 7, Water Kant Street, Capetown.
4.11 09	Smith, Austin Rogers, 18, Effingham Crescent, Dover.
6. 2 12	Skinner, E. H., M.D., 1018-1020, Rialto Buildings, Kansas City, Missouri, U.S.A.
	Soddy, F., M.A., F.R.S. (Honorary), Marischal College, Aberdeen. (*Past President.*)
6. 3 02	Stanesby, Reg. M., 350, Merton Road, Wandsworth, S.W.
	Stenning, H. J., 164, Upland Road, East Dulwich.
3. 1.07	Stowe, Wm. Reg., M.R.C.S., L.R.C.P., Linton Street, Palmerston North, New Zealand.
1.12.14	Stoney, Miss Edith A., 20, Reynolds Close, Hampstead Way, N.W.
1. 1.03	Stoney, Miss Florence Ada, M.D., B.S.Lond., 4, Nottingham Place, W.
97	Swinton, A. A. Campbell, F.R.S., M.I.C.E., M.I.E.E., 66, Victoria Street, S.W. (*Past President.*)
	Syme, Dr. W. H., Worcester Street, Christchurch, N.Z.
5.12.07	Talboys, F. P., 2, Brown Street, Dunedin, New Zealand; Box 203 Wangannie.
4. 2.13	Taylor, F. W., M.D., Kirkside, Preston New Road, Blackburn.
10. 6.98	Thomas, J. Lynn, C.B., F.R.C.S., 21, Windsor Place, Cardiff, and Greenlawn, Penylan, Cardiff.
	Thomson, J., Alexander, B.Sc., M.B., Ch.B., 18, Ripon Road, Harrogate.
3. 3.14	Thornton, G. E., M.B. Oxon., Grove House, Salisbury.
2.12.13	Trostler, I. S., M.D., 615, Garfield Avenue, Chicago, U.S.A.
97	Turner, Dawson, M.D., 37, George Square, Edinburgh. (*Vice-President.*)
1.11.06	Wall, Chas. P.B., M.D.Edin., "Butterworth," Transkéi, Cape Colony,
4.12.02	Walter, Rev. Fred. W., The Grange, Worstread, Norfolk.
	Waters, Dr. J. B., 39, North Bridge Street, Sunderland.
7. 2.01	Watson, Edward J., M.D., 25, Fitzwilliam Place, Dublin.
4.11.13	Watt, W. L., M.D., B.S.Lond., Winnipeg General Hospital, Winnipeg.
2. 3.15	Wellington, J. B. B., The Leys, Elstree.
5.12.11	Westlake, G. F., Cancer Hospital, Fulham Road, S.W.
7. 5.12	Whitcombe, W. S., M.D., 10, Lancaster Gate Terrace, W.
	White, Miss E. M., Northamptonshire War Hospital, Diston, Northampton.
5.12.01	Whitton, J., M.D., F.R.C.S. Edin., Oamaru, Otago, New Zealand.
4. 2.04	Whytt, Alex., M.D., 20, Clarendon Place, Stirling, N.B.
7. 2.01	Williams, John Robert, M.B., C.M.Edin., Ardre, Penmaenmawr, N.Wales.
3. 2.11	Wilson, Dr. Robert (Major C.A.M.C.), Office of Director of Medical Service, Canadian Contingents, Cecil Chambers, 86, Strand, W.C.
5. 3.12	Wilson, W. H., 12, Cobham Road, Norbiton, Surrey.
7. 2.01	Wilton, T., Winsor House, Beckton, E.
6.11.02	Worrall, Ed. S., M.R.C.S., L.R.C.P., 29, Queen Anne Street, Cavendish Square, W.
4. 1.00	Wright, R. S., 72, Wigmore Street.
	Young, F. B., 2, Eastfield Road, Cotham, Bristol.
	Yrigóyen, Dr., Cirraco, 33, Fuentenabia St., San Sebastian, Spain.

The PRESIDENT said that in connection with the resolution on the subject of the protection of the X-ray operator, which was passed at the last meeting of the Society, he wished to announce that they had had acknowledgments from the War Office and Admiralty. The War Office thanked the Society for their interest in the matter, and said that they were making use of the card containing the preliminary recommendations for protection.

Professor J. W. NICHOLSON, M.A., D.Sc., then read a paper on "The Homogeneity of Visible Radiation."

HOMOGENEITY OF VISIBLE RADIATION.

By Professor J. W. NICHOLSON, M.A., D.Sc.

In your address at the beginning of this Session, Mr. President, you chose the subject of spectra. Perhaps we are justified in returning to this more philosophical study after a year spent on very practical and urgent matters in the main. I hope to-night to give an account

of some recent work done by Dr. Merton and myself which, while concerned entirely, as it stands, with visible radiation as manifested in spectra, is not without suggestions of problems of a corresponding nature which come more completely into the scope of our Society, — perhaps I should use the future tense, for the study of X-rays, though they are light-waves of short wave length, has not yet reached the stage at which the present kind of problem can be discussed with much profit; we can only content ourselves with a realization of the ultimate problems which X-rays will present, by drawing a parallel between them and visible radiation as regards certain characteristics of the latter, which I am about to describe.

The title of my remarks conveys an attempt to draw this parallel. We all know what a homogeneous X-radiation means. It denotes radiation with a specified wave length, and I am venturing to apply the same term to visible light. For our present purpose, homogeneous visible light will mean absolutely monochromatic light forming an image, on the photographic plate, of the slit of a spectroscope. In other words, it denotes something purely ideal, for however narrow a slit may be used, any spectrum line actually seen or photographed has in all cases a definite breadth. Every spectrum line includes, in fact, an indefinite number of images of the slit side by side, formed by different wave lengths which lie in so far as they affect the photographic plate, within a certain narrow range, but can take any value within that range, so that the set of slit-images appears as a broad band. The intensity of the band shades to zero on either side, becoming zero when the light of the corresponding wave length is not strong enough to affect the plate. In fact there is a certain wave length, giving its image near the centre of the band, which is stronger than all the others, and it represents the natural vibration or " note " of the vibrating atom, produced from its interior. If the atom as a whole is moving, the apparent note is changed, as is the acoustic note of a moving railway whistle, Now in a collection of vibrating atoms of a gas through which a discharge passes, bodily motions of every magnitude are going on, and atoms with a specified velocity in the line of sight will give a note differing by a slight, but specified amount from the natural note. We get therefore a range of frequency or wave-length as in the band, and for the higher velocities, the number of atoms possessing them is small, and the corresponding wave-lengths are not strong enough to affect the plate.

We are, of course, at this point on familiar ground. Lord Rayleigh has in fact given a very complete theory of the broadening of spectrum lines into bands in this way, and the theoretical laws deduced are in accordance with experiment. They have even led to a knowledge of the relative masses of the atoms emitting the lines which would be monochromatic in the absence of bodily movement of atoms. But I have traversed the ground because the structure and breadth of spectrum lines is to be the subject of all my remarks,—and it may be said that the Röntgen ray may exhibit the same type of phenomena in a greater or less degree. This is for the future to decide. The nearest thing to homogeneous radiation that we can devise turns out to be in reality far from homogeneous when closely examined.

The broadening of spectrum lines described already is the so-called normal type, and is produced in the spectrum of any gas when excited in the ordinary way in a tube in which it is sufficiently vacuous. If the gas is denser, other effects come into play which are not our concern on this occasion. The normal broadening is produced by the normal discharge. When a condenser of variable capacity is put into the discharge circuit, we obtain the condensed discharge, and as the capacity is increased, the spectrum lines broaden in a quite anomalous way. Their breadth can become very great, and they do not always shade off equally on either side. Into the cause of these effects I propose to enter.

We are all familiar with the Zeeman effect, or the splitting up of a spectrum line into several different ones by the agency of a magnetic field. This phenomenon is fairly well understood on its theoretical side. But Stark and

Lo Surdo showed very recently—the war has naturally diverted attention from this remarkable phenomenon—that a very strong electric field can split a line into a very complicated set of individual lines in the simplest spectra we know. This phenomenon, which has at present no satisfactory explanation in theory, will without doubt lead ultimately to very precise information as regards the atoms of bodies and the mechanism of their emission of every type of radiation. It would take me too long to enter into details of individual cases, and it will not be necessary. But we have another indication that the homogeneity of emission of radiation by a body depends very much on circumstances. This is not even a case of a body preferring to emit one of its spectra rather than another, but represents an actual modification of one spectrum. The Hydrogen line Hγ, with two components in the ordinary way, has twenty-seven when a field of thousands of volts per centimetre is applied, and the separation between any two increases proportionally to this field.

Stark had suggested that this phenomenon perhaps lay at the root of the anomalous broadening under the condensed discharge,—that is to say, that under these conditions, the spectrum line was split up into many individuals, each being so broad—by virtue of atomic velocity, perhaps—that they all overlapped and gave a wide band. The experiments of Dr. Merton and myself were designed to test this point, in the first place, and more generally to obtain a method of accurate measurement of the distribution of intensity among the wave-lengths close together in one composite line under any conditions of excitement. Into the difficulties inherent in other suggested methods I have no time to enter. Perhaps, with other important points I must neglect, they may appear in the discussion afterwards.

The apparatus consists merely of the usual spectroscopic equipment with an accessory. This accessory is a neutral-tinted wedge, cemented to a similar wedge of clear glass, and mounted immediately in front of the slit of the spectroscope. The absorption of light by the wedge is not selective, and the wedge is so arranged that the slit is parallel to the direction of maximum increase of thickness of the wedge. The upper part of any spectrum line is therefore attenuated by the greater absorption of the wedge at this end, and as seen on the photographic plate, its length is shortened, terminating at the point where the thickness of wedge traversed is just sufficient to reduce the original intensity to the intensity which can just affect the plate, under the conditions of exposure adopted. The slit image corresponding to the most intense wave-length will be the longest, and the less intense wave-lengths give shorter images. A curve can be traced through the top ends of all the images, and it is this curve for which measurements can be made at leisure on the photographic plate, after development and enlargement, and as the law of absorption in the wedge is known, it is possible to calculate theoretically, from the shape of this curve, the law of distribution of intensity across the original spectrum line or band. Into the details or method of this calculation I shall not go, but it is sufficient to say that two main results can be enunciated. If a spectrum line is merely broadened by atomic velocities after the manner of Lord Rayleigh's theory, and consists of one component in the absence of this broadening, the curve obtained on the photograph should be a parabola, of smooth and regular curvature, sloping downwards equally on

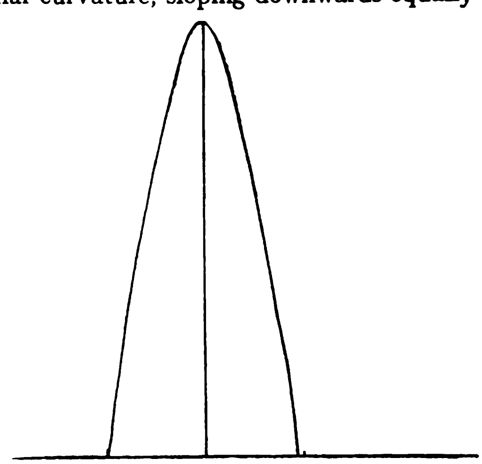
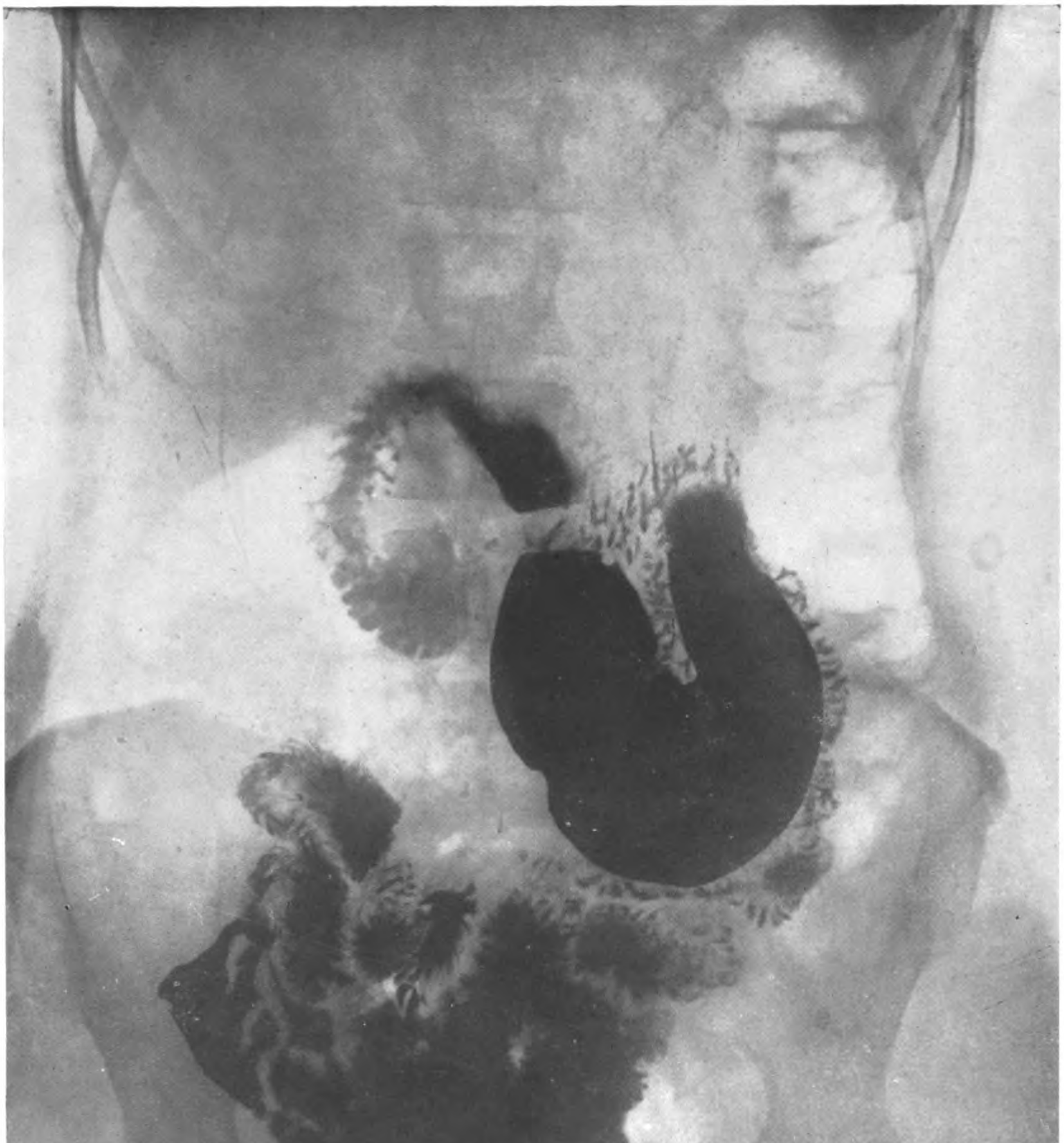


Fig. 1.

Ordinary discharge. Contour of a single line as photographed with the wedge and spectroscope. The contour is nearly parabolic.



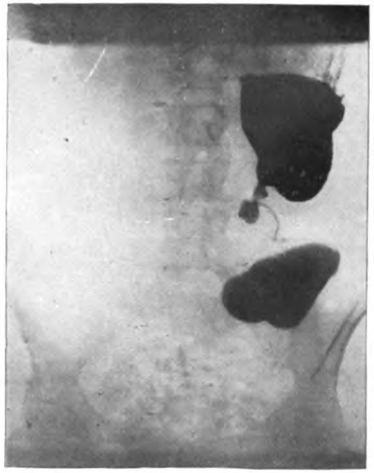
Opaque meal, showing food in stomach, duodenum and small intestine. Single flash exposure, intensifying screen.

PLATE IX.

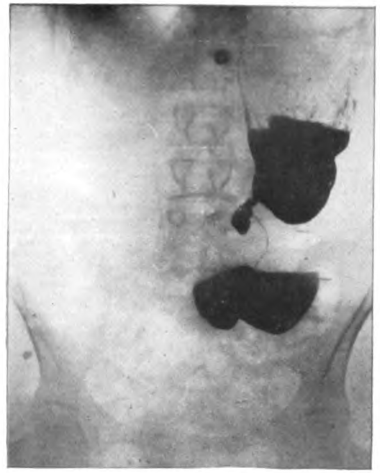
From the Radiographic Department, Cancer Hospital, London.

("Journal of the Röntgen Society."—Copyright.)

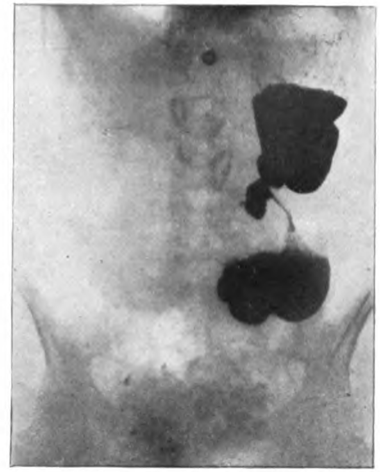
Opaque meal in an hour-glass stomach, showing upper and lower sacs, with a narrow channel between, penetrating ulcer on lesser curvature. Pyloric stenosis.



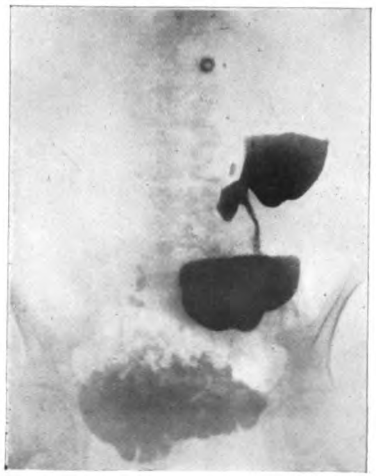
15 minutes after meal.



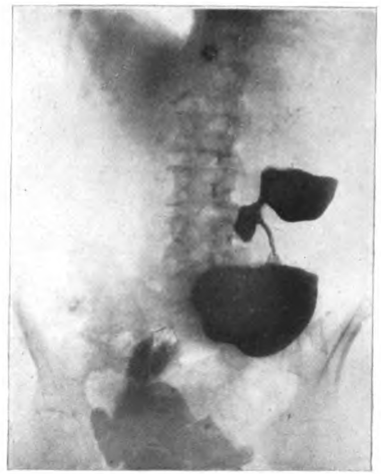
1 hour after meal.



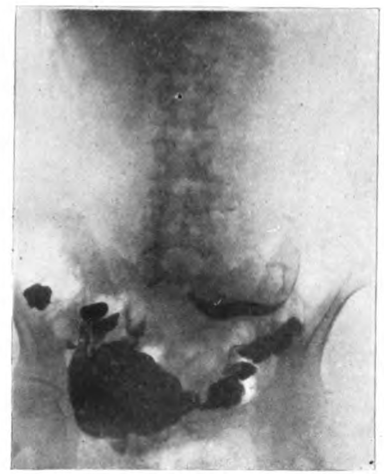
3 hours after meal.



5 hours after meal.



7 hours after meal.



24 hours after meal.

Single flash exposures, Tungsten target tube.

PLATE X.

From the Radiographic Department of the Cancer Hospital, London.

("Journal of the Röntgen Society."—Copyright.)

both sides of its apex, which should be rounded. If, on the other hand, the law of intensity distribution is the simple exponential one, not capable of production by the kinetic theory distribution of atomic velocities, every individual component should give a definite *peak* on the curve, from which it slopes down as a straight line on either side. If several broadened components overlap, as Stark suggested, a composite curve is obtained, which exhibits either a peak, or a protuberance more or less rounded, at the place where any component has its maximum intensity. These peaks and protuberances can be seen on the photographs by inspection, and the relative positions of the components of a composite line deduced from them by direct measurement on the plate.

The eye is notoriously inefficient as a detector of small differences of intensity on a blackened photographic plate. To attempt an accurate quantitative estimate of the intensity distribution across a band by any measurement of degree of blackening on a plate, and its variations, would be impracticable. The method described has the advantage of removing any such necessity. A variation of intensity in one direction is by this method balanced by a known variation in the perpendicular direction, and the boundary of the photograph is the assemblage of points where the balance occurs. Its shape therefore gives the unknown variation, and in particular at once picks out the places where any new line —even a weak one—is superposed on some part of the breadth of another. I have not found

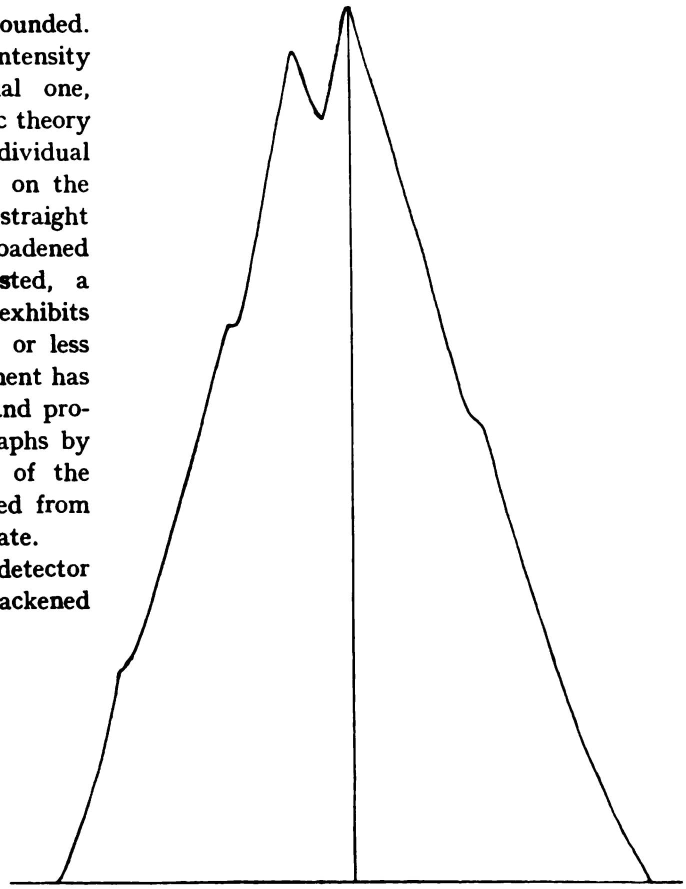


Fig. 3.

Condensed discharge. Typical figure of a line which is already double in the ordinary discharge. The number of kinks on the two sides may then be different. A helium type.

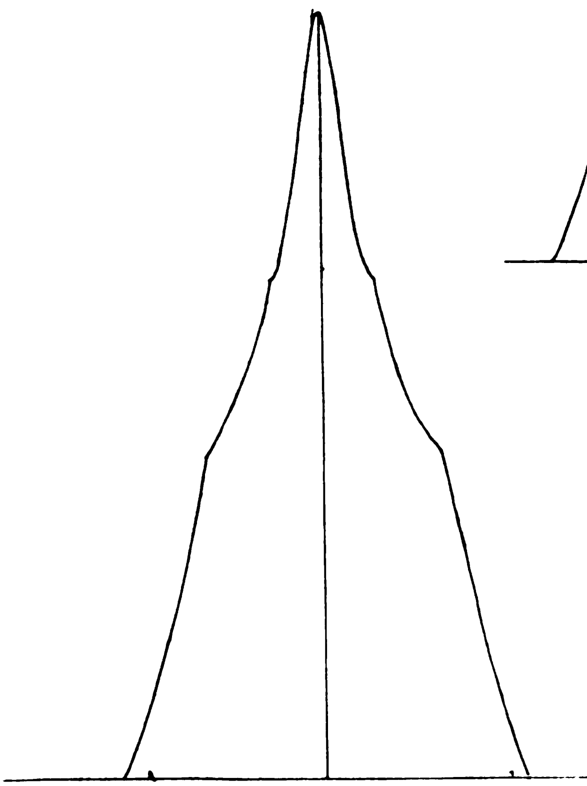


Fig. 2.

Condensed discharge. Typical photograph of a line which is single in the ordinary discharge. The kinks indicate axes of new components. The slight lack of symmetry is due to dispersion. Type H α.

it possible, as I had hoped, to have some of our photographs here to-night, but some figures on the board will bring out their characteristics.

The photographs obtained in this way with the condensed discharge are in complete agreement, both qualitative and quantitative, with our expectations on the basis of Stark's suggestion as to the cause of abnormal broadening. The separate components are present, as shown by the kinks in the boundaries; I refer more particularly to the Hydrogen line Hα, which we have examined most exhaustively, although other hydrogen lines, and those of Lithium and Helium which we have employed, fall into line. The ratios of the horizontal distances between these kinks are moreover the ratios of the distances between Stark's components, thus completing the proof. In the condensed discharge, therefore, the same splitting up of lines occurs as we get with a field of such an order as 30,000 volts per centimetre applied directly to the tube. For the separations between the components are comparable with those of Stark.

Each of these new component wave-lengths is broad enough to overlap its neighbours, but the breadth has quite another origin from the breadth of a line in the ordinary discharge. It cannot be ascribed to atomic velocities in the manner of a Doppler effect, for the distribution of intensity across each component follows quite a different law—the simple exponential. We are in presence of two phenomena, firstly, the existence of the different wave-lengths, and secondly, their curious broadening. One general characteristic is that they become broader—quite apart from their intensity—as they recede on either side from the central line as given by the ordinary discharge.

We have concluded that the origin of anomalous broadening in the condensed discharge has been found. It is the Stark effect, or electric Zeeman effect. But as I have said, no theory of a satisfactory kind has been put forward as to what the Stark effect means in connection with radiation, and I am unable to suggest one. It is only possible to maintain that whatever it means, we have another means of studying it in the condensed discharge. As to its appearance there, without the application of a high voltage, there is no difficulty if we take it that the atoms get closer together in their disturbance by condensed discharge, even to the extent of interpenetration. An electron actually passing through an atom can easily produce, as calculation shows, a *local* voltage of the necessary magnitude. In fact, the atoms are themselves made to do the work, instead of applying this work directly outside the whole assemblage.

I am afraid many points have only been touched on in this account and very imperfectly elucidated. But the subject of modifying the radiation from atoms is so wide that questions arise at every point, and I am therefore encouraged to think that any further details of our experimental method and its future application, together with the further interpretations of these preliminary results, are more appropriate for the discussion. I therefore leave them at this point.

DISCUSSION.

Professor A. W. PORTER said: I should like to congratulate the author of the paper upon his very lucid account of researches that have been initiated by himself and Dr. Merton at King's College. There is one point that I would like cleared up for my own satisfaction, and perhaps for the sake of others. I do not quite gather to what extent the results are complicated by the duplicity—the normal duplicity—of the hydrogen lines themselves. I would like to ask whether Professor Nicholson has experimented on a line which does not show duplicity, such as the red cadmium line. Professor Nicholson stopped just where the matter became most interesting, because what we want is his explanation of all these lines, and so far as I remember, he gave no indication whatever as to the possible explanation of the interesting observations that he has brought before us. I have nothing further to say just on the spur of the moment, save that once again I have to congratulate Professor Nicholson on the account he has given of the work.

Major ROBERT WILSON: I should like to

offer to Professor Nicholson the thanks of one who is not an adept at physics. I shall be pleased to see the paper in print, so that I may have the opportunity of considering it quietly. The thanks of the Society are due to Professor Nicholson for drawing attention to a most important factor in the X-ray. A homogeneous radiation is the philosopher's stone of the radiographer, and for eighteen months before this war started, Dr. (now Captain) Morgan and I were working with different sources of current, and feeling our way in the dark towards a homogeneous X-ray. I rather feel that, apart from the presence of gases in the tube, the source of current itself is very important. Such a current as we used to obtain from a static machine produced a radiation of a very different character from that which is possible to-day. In a radiograph of the frontal sinus, for example, taken sideways, we used to find with a static machine—or perhaps to a slightly less extent with a coil—that there resulted a clear-cut bone image, with very little of the flesh showing; but with the high-tension transformer we get a different type of ray, and now not only do the bones show perfectly and clearly, but we get the flesh, and in some instances on the same plate the very skin itself. I never used to get anything like this with my static machine. I do feel that if we can commence the study of which Professor Nicholson has only touched the outer-fringe to-night, we shall find much that is of practical value, and in the results of Professor Nicholson's researches along that line already we have something of much interest to go upon.

Capt. G. W. C. KAYE: I feel it extremely difficult to offer any reasoned criticism, but there is one outstanding thing which must be said, namely, that the bulk of the work which Professor Nicholson has dwelt upon to-night exemplifies the truth of the old saying that the more we learn the more there is which remains to be learned. Further work on such subjects as these invariably shows them to be very much more complex than the simple things we imagined at the outset. In the early days when Professor Barkla discovered homogeneous X-rays, he laid it down that every element had at least one homogeneous X-ray of its own. In some cases there proved to be two, one of which was named the K radiation and the other the L radiation. This was all very delightful, but then came along the crystal workers, and we found that the radiation was by no means homogeneous. Some have found three, four or five prominent lines in the spectra, and further work has proved that there may be eight or ten lines, or possibly more. Professor Nicholson will end, I am afraid, by confronting us with an appalling number of lines, and we shall find our hopes of homogeneous radiation sorely disappointed. With regard to the remarks of the last speaker, I would just like to say that previous to the war I was engaged in a scheme of X-ray analysis, namely, the analysis of the X-ray beams obtained from a hard tube under ordinary conditions. The work was stopped owing to the war, but enough was done to show that, unless we alter our conditions, our chances of getting a homogeneous radiation are extremely small. The proportion of L radiation obtained out of an ordinary tube under ordinary working conditions is trifling, perhaps 5 per cent. If the tube be run soft—so soft, probably, that it is not of much use even for therapeutic work—one may get a fairly large proportion of homogeneous X-rays. My experience has been with platinum, and in some cases with tungsten, that no matter how much you cut your rays down—you may cut them down so that the strength is about 1/1000th of what it was at the beginning—the X-ray which gets through, although nearly homogeneous, is not quite so.

Dr. SIDNEY RUSS: I feel that it has been a great treat for the members of the Röntgen Society to listen to Professor Nicholson to-night, and I venture to ask him a very simple question. He touched upon the possibility of the electrical conditions in the circuit affecting X-ray radiation, just as it is shown to affect visible radiation. He has told us all too little of the theory he holds on this question, but can he tell us whether he would expect the necessary

electrical force to be greater or less than those he uses for his optical experiments? The question is often put as to the possible variation in the X-ray radiation according to the stimulus employed in the bulb. He would help us considerably by showing us what possibility there is of anything of the kind happening.

Professor J. W. NICHOLSON: Professor Porter has asked me three very interesting questions. In the first place he pointed out that the ordinary hydrogen lines are themselves in normal circumstances double to start with. The two components of these doublets are very close indeed, so close that the separation between the two is not known with any extreme accuracy. Different investigators give it as ·20, as ·4, as ·132, and as ·065 in the first line, and these might be called the four national values, because the nationality of the investigators in each case is different. When we come to compare these numbers we see what a great degree of uncertainty attaches to the separation of these components. Michelson gives the ordinary intensity ratio of the two components as 7 to 10, and they overlapped always in every attempt which was made to produce them. Well, the separation in the next line is still smaller, and there is considerable doubt as to how much smaller. Professor Porter asked whether this initial complication might have something to do with the present complication in the results. In the present case it has not very much to do with the *normal* broadening in the ordinary discharge. When we come to the condensed discharge, I find it difficult to say to what extent the complication is due to one component and to what extent to the other, but I have a feeling that the initial doubling is not important. For it is small in comparison with the separations we find here, and the helium lines, even when *single* in the ordinary discharge, are very complex when showing stark effect. At the same time it is, of course, conceivable that the final structure we get for hydrogen lines depends a little on their double nature. I will take Professor Porter's third question next. He invited me to say what theory I had as to what was behind these results. I must confess I have met none which inspires confidence. There are at least two conflicting views, which both seem wrong. I deliberately confined myself to registering experimental results on this account, and although I have dabbled a little in the theory of these things, I have not ventured to come down on either side of the fence. Professor Porter asked also whether we had examined any line which is not definitely known to be double to start with. Well, we have not as yet. The lines we have really examined are the hydrogen lines and certain of the helium lines and lithium lines as well. But since the broadening of these is evidently due to the stark effect, there is every reason to believe that the same applies to the single lines. But as soon as we ask whether initial doubling alters the number of components, we are asking for the theoretical basis of the stark effect. Our own work merely indicates another case in which the stark effect operates, and all I can really give by way of theory is the remark I made originally that so far as the theory of our experiments is concerned, we rather prefer to limit it to this: that in some way in these experiments a field is produced in the neighbourhood of any individual atom which is of the same order of magnitude as that which is applied externally to the system by Stark in his experiments, although we do not apply a huge potential gradient from the outside. Such a field is produced, theoretically, when charged atoms get so close together as almost to penetrate each other. It is very easy to prove that fields of that order of magnitude would be produced by a direct calculation from atomic data.

I must thank Major Wilson for the kind remarks he made. I am very glad to infer from him that I have succeeded in making myself to some extent clear. It is very difficult to make these matters clear to a person who is not a professed physicist. The subject which Major Wilson introduced was to a great extent answered by Captain Kaye. With regard to different modes of exciting the radiation, in a sense we might say that the change of radiation with the mode of exciting was very much the

point of these experiments. The results are, in fact, a comparison between the ordinary structure of the spectrum lines of hydrogen, which is simple, and the structure under these new conditions, when it becomes exceedingly complicated. It is entirely, from one point of view, a question of the mode of excitation. Now the question of homogeneity in connection with X-ray work is not so much the homogeneity of one particular " line," but the homogeneity of the spectrum. On that subject I hope I shall be able to say something to the Society within the next year. The question of the transfer of energy from one line to the next number of a series to which it belongs, and the conditions of excitation which govern it, is a thing for still more investigation. We have no certain results I would care to mention now, but I promise the Society that it shall hear something about that later on, and that is, of course, the point which is more directly important in practice when we want to talk about X-rays. I chose the title of my paper, I may say, as a compromise. I wanted to talk about visible radiation, and yet to convey some idea of what the work might lead to in connection with X-rays.

There is one practical detail to be noted, namely, the method by which these contours were delineated precisely. I think it would interest the Society. They were not photographed in the ordinary way straight on to a plate, but through a process screen ; that is to say, a plate ruled with lines at about 100 to the inch in two perpendicular directions, so that all these delineations are really composed of very small dots extremely close together, and forming a regular pattern. Anyone who has ever photographed the spectrum knows the importance of picking out the extreme boundary of visibility, but it is a perfectly possible thing to prick out the last black dots you can see all round the edge of this arragnement. We actually prick out the last dot in any direction, and in that way we believe we have practically eliminated the personal equation. And it is noteworthy that the results we get with this method are much more accurately in correspondence with theory than those we get by the first method, which consists of tracing the boundary on an ordinary photograph.

I sympathize with Dr. Kaye in finding continually that all the phenomena we thought simple are much more complicated than we ever dreamed of, and this applies very closely to the structure of X-rays, which is almost certainly as complicated as any of them. We intend before very long to attempt to apply these methods to the X-ray. At any rate, we are going to try and attack some definite problem of the structure or precise measurement of the wave length of the X-ray very shortly.

Dr. Russ wanted to know what theory I hold about these matters. I have said already what I want to say about that. He also asked whether I could give any idea as to whether the electrical force needed to break up an X-ray in this manner would be greater or less than the force required to break up the visible radiation. If we take the more limited ground that the resolution will be produced in these experiments when a force of the right magnitude is applied, whether applied directly from outside or otherwise, then what we may say is this : if this phenomenon is really due to the neighbouring atoms getting close up to the atom which is emitting the radiation, and if we take the usual view held now, that is to say, the view of Professor Barkla, who recently discovered the characteristic Röntgen radiation, that the Röntgen radiation comes from some region much more internal to the atom, the force which would be required to drive the outward part of some other atom sufficiently close to affect the portion which produces Röntgen rays would be larger, and in that case we should be tempted to say offhand that the electrical force would have to be larger, but not so considerably larger as might be thought the case, as we find if we calculate the electrical force between two charges in an atom. The force is very large indeed, and goes up very rapidly indeed, too. The one atom might just as easily get within the necessary distance of the other with pretty much the same force as those in question to-night. I think that deals with all the questions which have been submitted, and, in conclusion, I must

thank the Society for listening to me on a very unusual topic with so much patience.

Dr. G. B. BATTEN: At this last meeting of the session we cannot go away without passing a vote of thanks to Mr. Gardiner, our President. During the present year, although many societies have suffered very greatly, our own Society has had a very successful time, and this has been very largely due to the able and careful judgment of Mr. Gardiner in the chair. He is not one of those pushing personalities who demand attention, but we, who have known him for twenty years or longer, esteem his worth. I include in the vote the other officers, and the Secretary and Treasurer.

Dr. RODMAN seconded the vote of thanks, which was accorded by acclamation.

The PRESIDENT said, in acknowledgment, that this matter had taken him entirely by surprise, and he could only thank the members very heartily for the patient way in which they had borne with him in the chair. If there was any credit due for the success of the Society to any one individual, it was due to their Honorary Secretary, Dr. Knox, who had put in a great deal of hard work that few knew anything about.

There was an important and valuable collection of electrical and X-ray apparatus. Demonstrations were given by various members of the Society, many new devices were shown. Below will be found a list of the exhibitors and exhibits in alphabetical order.

CUTHBERT ANDREWS.

Various models of Rapid BRITISH MADE X-RAY TUBES, including Mammoth and Water-Cooled Tubes with Tungsten Target, Valve Tubes, etc.

Also the latest patterns of Rubber Protective Aprons, Masks and Gloves, all of British manufacture.

The Walter pattern Radiometer for determining by visual means the penetration of X-ray Tubes.

T. CLARK & SONS.

An eight-plate Static Machine, giving 11 inch spark, with accessories, for therapeutic application. The use of this machine was demonstrated by Mr. Clark and caused much interest.

An X-ray Examination Couch and Indicator.

An overhead Protective Tube-box, with scales, for stereoscopic radiography.

Negative Viewing Box, with light diffusing arrangement.

Portable Light-baths.

HARRY W. COX & CO., LTD.

16-inch Heavy Discharge Coil.

Cox Standard Couch, 1916 model, with lead-covered tube-box, provided on ball bearing trolley, and stereoscopic shift and locking devices are fitted to both movements.

Cox's Chromo Radiometer for determining the colour changes which occur in the Sabaraud Pastille.

Selection of London Fluorescent and Intensifying Screens.

Book of Dr. Levy's Pastilles.

Stenning Localizer.

NEWTON & WRIGHT, LTD.

Improved Model Instanta Interrupter with Tachymeter attachment and auto cut-out switch for therapeutic work.

The London Qualimeter (Bauer Patent, manufactured under special license).

Perfector Model No. 2 Tube Stand, with set of fittings for Therapy and Radiography.

Rectangular Diaphragm, controlled by Bowden wire mechanism.

"Triplex" Exposure Switch.

Improved Model of Dr. Hampson's Radiometer.

Pneumatic Tube Regulator, constructed to adjust vacuum of tube when it is working.

"Instanta" X-ray Tube, with platino-iridium target.

"Instanta" Valve Tube, constructed in brown-coloured glass to cut down luminosity.

MESSRS. SCHALL & SON.

The Pantostat.
Tesla Interrupter.
X-Ray Couch, with tube-box, in ball-bearing framework.
Smart and Breston Faradic Coil.
Otheoscope.
Lewis-Jones Condenser Muscle Testing Set.
Combined Battery and small Faradic Coils.

NOTES.

THE CONSTANTS OF RADIOACTIVITY.

KOLOWRAT'S table of radioactive constants was published by *Le Radium* in 1914. This journal having been suspended on account of the war, the constants have been re-tabulated and brought up-to-date by Gerald L. Wendt. The following table is published in the *Physical Review* for March, 1916.

Substances.	λ (Sec.$^{-1}$)	P	Chem. Group.	Rays.	$R_{15°}$ (Cms.)	$\mu\beta$(Al) (Cm^{-1})	$\mu\gamma$(Al) (Cm^{-1})
Uranium 1	$4.3 \cdot 10^{-18}$	$5 \cdot 10^{9}$ yr.	6	α	2.50		
Uranium X_1	$3.3 \cdot 10^{-7}$	24.6 d.	4	β		510	24; 0.70; 0.140
Uranium X_2	0.01	1.15 min.	5	β		14.4	
Uranium 2	$1.1 \cdot 10^{-14}$	$2 \cdot 10^{6}$ yr.?	6	α	2.90		
Uranium Y	$7.5 \cdot 10^{-6}$	25.5 hrs.	4	β		300	
Ionium	$2.2 \cdot 10^{-13}$	10^{5} yr.	4	α	3.11		
Radium	$1.26 \cdot 10^{-11}$	1730 yr.	2	αβ	3.30	200	354; 16; 0.27
Ra Emanation	$2.085 \cdot 10^{-6}$	3.85 d.	0	α	4.16		
Radium A.	$3.85 \cdot 10^{-3}$	3.0 min.	6	α	4.75		
Radium B	$4.33 \cdot 10^{-4}$	26.7 min.	4	β		75	230; 40; 0.51
Radium C_1	$5.93 \cdot 10^{-4}$	19.5 min.	5	αβ		13.5	0.115
Radium C_2	$8.3 \cdot 10^{-3}$	1.4 min.	3	β			
Radium C′	$7 \cdot 10+^{5}$	10-^{6} sec.?	6	α	6.94		
Radium D	$1.39 \cdot 10^{-9}$	15.83 yr.	4	β		130	45; 0.99
Radium E	$1.66 \cdot 10^{-6}$	4.85 d.	5	β		43.3	like Ra D
Polonium (RaF)	$5.90 \cdot 10^{-8}$	136 d.	6	αβ?	3.84		585
Thorium	$1.2 \cdot 10^{-18}$	$1.5 \cdot 10^{10}$ yr.	4	α	2.72		
Mesothorium 1	$4.0 \cdot 10^{-9}$	5.5 yr.	2				
Mesothorium 2	$3.1 \cdot 10^{-5}$	6.2 hr.	3	β		30	26; 0.116
Radiothorium	$1.09 \cdot 10^{-8}$	2.02 yr.	4	α	3.87		
Thorium X...	$2.20 \cdot 10^{-6}$	3.64 d.	2	α	4.30		
Th Emanation	0.0128	54 sec.	0	α	5.00		
Thorium A	5.0	0.14 sec.	6	α	5.70		
Thorium B	$1.8 \cdot 10^{-5}$	10.6 hr.	4	β		110	160; 32; 0.36
Thorium C_1	$1.9 \cdot 10^{-4}$	60 min.	5	αβ	4.80	16.3	
Thorium D	$3.7 \cdot 10^{-3}$	3.1 min.	3	β			0.096
Thorium C′...	$7 \cdot 10+^{10}$	10-^{11} sec.?	6	α	8.60		
Actinium	$1 \cdot 10^{-10}$	200 yrs.?	3	α?	3.56		
Radioactinium	$4.25 \cdot 10^{-7}$	18.88 d.	4	αβ	4.2	170	25; 0.190
[Radioactinium′]?	$3.2 \cdot 10^{-6}$?	60 hr.?	?	α?	4.61		
Actinium X	$7.6 \cdot 10^{-7}$	11.4 d.	2	α	4.26		
Act. Emanation...	0.18	3.9 sec.	0	α	5.57		
Actinium A	350	0.002 sec.	6	α	6.27		
Actinium B	$3.2 \cdot 10^{-4}$	36.1 min.	4	β		soft	120; 31; 0.45
Actinium C_1	$5.37 \cdot 10^{-3}$	2.15 min.	5	αβ?	5.15		
Actinium D	$2.26 \cdot 10^{-3}$	4.71 min.	3	β		28.5	0.198
Actinium C′	700	0.001 sec.	6	α	6.45		

The following are general relations involving these constants:

$\lambda P = ln 2 = 0.69315$.

$\log P = A + B \log R$, where A and B are constants for each series.

$R_{T_1P_1} = R_{T_2P_2}((T_1P_2)/(T_2P_1))$, where T is the absolute temperature and P is the pressure.

The original memoir can be obtained on loan by application to the Hon. Editor.

APROPOS of radium bearing minerals other than pitchblende, within the last few years rare minerals have been discovered in Madagascar, notably a deposit of uranium niobium and tantalum occurring in different varieties that have been named Betafite, Samiresite, Ampangabeite and Bloomstrandite, from the localities in which they have been found. The uranium content of these minerals is 26%, 21%, 19% and 18% respectively. Very little is known about these interesting bodies, but they are said to exist in considerable quantities, and may possibly come into greater prominence as a source of radium in the near future.

An authoritative account of the minerals, with chemical analysis, is given by M. A. Lacroix, in the *Comptes Rendus* for April 22nd, 1912.

IN the discussion following Professor Nicholson's paper on the "Homogeneity of Visible Radiation," remarks were made on the increasing complexity of the X-ray spectra of some elements. In the *Physical Review* for April, 1916, there appears a note by Arthur H. Compton on the X-ray spectrum of tungsten, with a list of thirteen lines between $\lambda 1 \cdot 0387$ and $\lambda 1 \cdot 5044$ in the L radiation of this element. *(See also Abstract* 894.)

IN the *Journal of Industrial and Engineering Chemistry*, viii., No. 3, there appears an interesting article by Charles H. Viol upon the recent production of radium salts from Carnotite, carried out at the laboratories of the Bureau of Standards, Washington. The author is somewhat critical as to the methods used and of the estimated cost of extraction; these are matters that do not greatly concern us, but it is interesting to note that the Bureau of Standards have carried out the practical extraction of the radium from 1,000 tons of ore. The material used was from the deposits in Colorado and Utah, and carried from 2·5 to 2·6 per cent. U_3O_8. These are classed as high-grade ores, low-grade averaging 0·8 to 1·0 per cent. U_3O_8. (The analysis of pure Carnotite given by the Imperial Institute, London, is 60 per cent. U_3O_8.) Most of the radium salt was worked up to a sufficient state of purity to be available for therapeutic purposes. The vanadium contained in the ore is a valuable bye-product on account of its use in the production of vanadium-steel.

MESSRS. FREDK. R. BUTT & CO., LTD., 147, Wardour Street, London, W., have sent us a copy of their recently issued catalogue, Section "B," of tubes and accessories for radiography and X-ray treatment. The catalogue is very complete and well illustrated; it is novel, in that it contains full notes on the management of X-ray tubes, details of the localization of foreign bodies, etc. These latter are particularly good, and the very numerous original drawings that are used help greatly to make clear the simple laws underlying the various systems of localization now in use; this section occupies some thirty-six pages. Illustrations and particulars are also given of many types of British and American X-ray tubes.

NEW BOOKS.

THE EMISSION OF ELECTRONS FROM HOT BODIES. By *O. W. Richardson, F.R.S., Wheatstone Professor of Physics, King's College, London.* Longmans, Green and Co., 39, Paternoster Row, E.C. Price 9s. nett.

The above volume, another contribution to the series of "Monographs on Physics," edited by Sir J. J. Thomson, O.M., F.R.S., and Frank Horton, Sc.D., is a welcome addition to the literature of radio-physics. Professor Richardson's researches on electron emission are well known, and no one is better fitted to present a *résumé* of this important and *novel* subject. For many years isolated observations have been made upon the conductivity produced in air in the neighbourhood of hot bodies, but until the discovery, by Sir J. J. Thomson and others, of the electron and of the laws governing its movements, these observations were more or less disconnected and difficult of explanation. After an introductory chapter dealing with "considerations of a general character," the theory of the emission of electrons from hot bodies is set out in a masterly manner in Chapter II.; the following chapters are devoted to the physics of the subject under all known conditions in a way that is only possible by one who has a thoroughly practical knowledge of it. Naturally many original experimental devices and methods are shown, and although the mathematical side of the question is developed, it is limited strictly to what is necessary.

The author in his preface explains that he considered it undesirable to include an account of the technical developments and practical application of the subject; it would certainly be impossible to do so within the compass of a single volume. The various devices for rectifying alternating currents, for the production of constant high potential, and the production of X-rays, as in the "Coolidge" tube, are a few of the results following the clearer understanding of "Electron Emission," and Professor Richardson's book will be found invaluable to all those whose work and interest lie in this direction.

A PRACTICAL GUIDE TO X-RAYS AND ELECTRO-THERAPEUTICS AND RADIUM THERAPY. For Students and Practitioners. By *A. E. Walter, M.R.C.S., L.R.C.P., Major Indian Medical Service, Superintendent X-Ray Institute of India.* Dehra Dun Thacker, Spink & Co., Calcutta and Simla.

Before the war we might have considered that in the present state of radiography and electro-therapeutics, a treatise of this kind, dealing as it does with the elementary principles of electricity and physics, was a little out of place. But the war has brought about many changes, and in the domain of radiology, which has become of such vital importance, the change is very marked. The sudden demands of the wounded have forced the practice of radiology upon a vast number of people whose previous knowledge of electro-physics was either very slight or absent altogether.

In this abnormal state of things, Major Walter's book is not at all out of place, and we can congratulate the author upon having introduced the subject in this wide and general manner. To many, especially to nurses and assistants who suddenly find themselves engaged in the manipulation of the strange and powerful apparatus, a perusal of the earlier part of this book will be of the greatest value for it will give them an intelligent appreciation of the work in which they are engaged, and warning of the dangers to which they are exposed.

The latter part of the book is occupied largely with diagnosis and treatment. Much good information is given upon the effects produced by X-rays, radium, and other electro-therapeutic agents. Here is found the practical experience of one who, being far removed from the great centres of activity, has had to depend very largely upon his own observations.

The book is profusely illustrated, very many well-known cuts from "Ganot's Physics," and from the catalogues of prominent instrument makers are noticed;

some of the half-tone reproductions are decidedly poor in quality. There is a thoroughly good index, making reference very easy.

On the whole Major Walter has succeeded in bringing together a large amount of valuable information within a small compass.

Localization by X-Rays and Stereoscopy. By *Sir James Mackenzie Davidson, M.B., C.M. Aberd.* H. K. Lewis & Co., Ltd., 136, Gower Street, London, W.C. 7s. 6d. nett.

At the present juncture, when all the power that recent developments in our knowledge in radiology has given, needs to be applied to the best advantage, a book on X-ray manipulation by such an authority as Sir James Mackenzie Davidson must of necessity be of very great value. As the originator of precise methods of localization and a strong advocate of the application of the stereoscope to X-ray interpretation, the author has done more than anyone to place the science of radiography in its adaptation to surgical diagnosis upon a sound scientific basis.

The object of the book is to offer the service of ripened experience to "those engaged in X-ray work among the wounded." It is not a treatise upon radiology, but in it will be found a fund of trustworthy information of very great value to those actively engaged in our over-crowded hospitals and field installations.

Except for a brief description of the X-ray tube and of the phenomena accompanying the production of the rays, no attempt is made of teaching electro-physics, the author's sole object being to place his personal experience at the disposal of those now engaged in the work. Before dealing with manipulative details, the subject of X-ray dangers and "protection" is very wisely introduced, and warning is given of the probable harmful effects of secondary or vagrant radiations, as distinct from those due to the X-ray beam *per se*. Detailed instruction is given, so that operators may observe for themselves that radiations are given off from practically the whole body of the tube, and the obvious conclusion is reached that the best method of protection is, in the first place, to shield the tube with a material through which X-rays cannot pass, except at the working aperture, and in the second to avoid all unnecessary exposure to the radiations.

The subject of precise localization is very fully expounded, and workers who are at present employing X-rays for localization of foreign bodies will do well to give these chapters their careful attention.

The special subject treatment of the eyeball and orbit, in which the author undoubtedly takes the first place, is very fully developed.

An appendix treats with the more recent application of the telephone and the electro-magnet as aids to localization, and a chapter is devoted to rectifiers and the various methods in use for obtaining unidirectional currents. Finally, the author has reprinted the "Recommendations" for the protection of X-ray operators recently issued by the Röntgen Society.

A novel and very important feature in the book is the inclusion of a number of half-tone stereoscopic pictures; these can, if desired, be removed from the book, and instructions are given for the construction of a simple and efficient stereoscope for viewing them.

This forms a most interesting collection of pictures, and their examination will do more than any amount of writing to convince the surgeon of the advantage of the stereoscopic method of working.

In addition to its intrinsic value, the book has a special charm in carrying with it the personality of the author. To those who have the pleasure of his acquaintance, the instructions and directions given are so characteristic as to be almost audible, and the lucidity for which he is noted is fully maintained throughout.

ABSTRACTS.

The following are selected from the current numbers of "Science Abstracts" as likely to be of special interest to members of the Society, and are published by permission of the Editors of that Journal.

726. *The Bases of Dosimetry in Radiology.* R. Ledoux-Lebard and A. Dauvillier. (Comptes Rendus, 162. pp. 405-407, March 13, 1916.)—The determination of the dose of Röntgen rays used in radiotherapy is a very complex question. The different methods which, up to the present, have been employed all present marked selective effects, and consequently require, first of all, qualitative measurement. They give only a mean value; some of them are inconvenient, and not one of them enables a precise measurement of the intensity or quality of the radiation to be made, because the latter is always very heterogeneous. If it is desired to introduce into radio-therapy rational and incontestable means of determining the dose, it becomes necessary first of all to modify the sources of the rays by making tubes which give a monochromatic radiation, the quality and intensity of which can be immediately measured with exactitude. It is further necessary to modify the sources of high-tension current such as are at present used. It is proposed to employ a continuous current for tubes used in radio-therapy, particularly in the case of those fitted with a Coolidge kathode. A Coolidge tube, run with a continuous current supplied from a condenser charged by a coil or a turning contact, gives a bundle of rays absolutely continuous; the phenomenon of discontinuity of kathode emission does not exist in such a tube. Further, the fact that the intensity is independent of the voltage makes it possible for the tube to be mounted directly on the terminals of the condenser. Ordinary and Coolidge tubes, run with continuous current, show no difference in the power of penetration of the rays from those obtained directly by means of a coil. The rays have a hardness of 8·5° Benoit with an equivalent spark of 16 cm., which corresponds to a static potential of 80,000 volts. After some difficulty, condensers have been made which support easily a pressure of 100,000 volts per element, and by joining them up in cascade it is possible to make them good for 200,000 volts. Under these conditions the apparatus gives exact indications which are easy to interpret, and it is possible always to obtain identical conditions. A. E. G.

727. *Illusory Protection against Röntgen Rays—Physical Anaphylaxis.* J. Bergonié. (Archives d'Él. Médicale, 24. pp. 111-112, April, 1916. Comptes Rendus, 162. pp. 613-614, April 17, 1916.)—It appears that tissue that has once been affected by Röntgen rays remains for a long time more or less sensitive in respect to those rays. It has been found that while in the case of a normal skin in order to provoke a sensible reaction it takes a dose which corresponds to a 15-minute exposure at a distance of 20 cm. of the kathode of a Chabaud-Villard tube emitting rays of 6° Benoit and run by a current of 0·5 milliamp., in the case of a sensitive subject an exposure measured in seconds is sufficient, and the distance is measured in metres. In fact, in a hypersensitive subject a dose of 1/1600 of that normally required will produce a notice-

able reaction. This reaction which follows such extremely small doses cannot be attributed to suggestion. Certain well-marked changes in the tissues, and other effects, are described. A. E. G.

808. *Absorption of Gas by Quartz Vacuum Tubes.* R. S. WILLOWS and H. T. GEORGE. (Phys. Soc., Proc. 28. pp. 124-130; Disc., 130-131, April, 1916.)—The experiments are a continuation of those of Willows [Abs. 1513 (1901)] and Hill [Abs. 486 (1913)] on the absorption of gas which is brought about by electrical discharges. A new quartz bulb does not absorb air, but if it be fed with repeated doses of hydrogen—which are absorbed when an electrodeless discharge is passed—it then becomes very active. If discharges in hydrogen are alternated with those in air the bulb can be made to absorb large quantities of either gas, and the activity with each gradually increases. The authors reject the theory of surface absorption, and, in their own experiments at least, also Swinton's theory that the gas is shot into the walls and held there. It is supposed that chemical actions occur with air, and oxidation products are formed; these are reduced by hydrogen. The process is compared with the formation of the plates in a Planté cell; the absorption of hydrogen corresponding to the charging, and that of air to the discharging of the cell. Attempts to produce the same effects by chemical treatment were partially successful, particularly in fatiguing the bulb so that no further absorption took place. The conditions under which the primary and secondary hydrogen spectra appear are also described. AUTHORS.

894. *Extremely Penetrating Radiations of the K-series of Tungsten, and the X-ray Spectra of Heavy Metals.* DE BROGLIE. (Comptes Rendus, 160. pp. 596-597, April 17, 1916.)—With the increasing use of Coolidge tubes the author gives a few results which are of interest to users of this tube. The observations prove the existence of a group of rays of extremely high frequency which correspond to the K-radiation of tungsten. This group comprises:—

Rays.	"Selective Angle" from NaCl.	Wave-length.
α_K	2° 04′	$2{\cdot}032 \times 10^{-9}$ cm.
β_K	1° 48′	$1{\cdot}768 \times 10^{-9}$ cm.

The "ray" α is a doublet of which the components are separated by an angle of about 2′. These extremely penetrating X-rays may be classed in the same spectral region as the penetrating γ-rays from RaB. They are produced when the p.d. reaches 75,000 volts.

An examination has also been made of the X-ray spectra (by an analysis of secondary X-rays) of the heavy metals Th and Ur. Similar observations on Hg, Tl, Pb and Bi have been published previously [Abs. 1837, 1838 (1914)]. For Th and Ur, the rays noted are:—

Thorium (oxide) ...7° 47′ (strong), 8° 03′, 9° 44′ (double),
Uranium (oxide) ...7° 22′, 7° 43′, 9° 18′,

which give, by Moseley's law, the atomic numbers 90 and 92 for these elements. A. B. W.

926. *Absorption of Gases in Vacuum Tubes and Allied Phenomena.* S. BRODETSKY and B. HODGSON. (Phil. Mag. 31. pp. 478-490, May, 1916.)—Experiments made under various conditions indicate that the absorption of gases in vacuum tubes is probably a phenomenon of some complexity and that, with metallic electrodes, the effect is due principally to absorption of gas by the disintegrated metal, although it may depend partly on the liberation of the alkali metals by electrolysis of the glass and chemical combination of these metals with the gas. Many of the apparently contradictory results obtained by different experimenters are reconcilable by means of such dual explanation. Experiments in tubes of materials other than glass would be of interest. T. H. P.

944. *The Lilienfeld Tube for Röntgen Rays.* F. J. KOCH. (Dresdner Elektrot. Verein, 10, No. 10, p. 85. Elekt. Zeits. 37. pp. 185-186, April 6, 1916.)—Lilienfeld found that up to a certain degree the occurrence of a discharge is independent of the goodness of the vacuum when a glowing kathode is employed in the tube. The special form of Röntgen tube designed by Lilienfeld, in which an ordinary metal-filament lamp is used as a glow-electrode, is here described with details as to working. There is also a brief account of a valve tube for obtaining unidirectional current, constructed on the Lilienfeld principle with a glow kathode. A. E. G.

EXTRACTS FROM THE TECHNOLOGIC PAPERS OF THE BUREAU OF STANDARDS.

The following are summaries of scientific papers published by the BUREAU OF STANDARDS, WASHINGTON.

The Editor will be pleased to send the originals of any of these papers to members of The Röntgen Society upon application.

Studies of Instruments for Measuring Radiant Energy.—The present paper gives the results of an investigation of the behaviour of a bismuth-silver thermopile suitably modified to measure radiant energy in absolute value. Instead of exposing the thermopile directly to the incident radiation a blackened metal strip intervenes. This metal strip functions (1) as a receiver for absorbing radiant energy; (2) as a source of radiation (by heating it electrically), which can be evaluated in absolute measure, and by using a constant current for heating the strip; (3) as a standard source of radiation for testing the sensitivity of the radiometer, which includes both galvanometer and thermopile.

The present investigation pertains to thirteen receivers, made of manganin, "therlo," and platinum, differing in width from 2·5 to 8 mm., and in thickness from less than 0·001 mm. for platinum to 0·011 mm. for manganin.

The manganin and "therlo" receivers were painted with a thick coat of lampblack, then smoked. The platinum receivers were covered with platinum black and afterwards smoked. In this manner (the same values being obtained in the two cases) it was shown that there is but little difference in the reflecting power of these two kinds of absorbing surfaces. (See receiver No. 10, Table 6.)

Beaumé Hydrometer Scales.—The paper was prepared for the purpose of clearing up certain erroneous ideas in regard to the origin of the Beaumé scales in use in the United States.

Special attention is devoted to the Beaumé scale for liquids lighter than water, and the reasons are given to show why the scale based on the modulus 140 should be used to the exclusion of all others.

Protected Thermoelements.—The paper describes in detail a convenient mounting for protecting laboratory thermoelements from danger by contamination or by mechanical strains. This mounting was developed in the thermal expansion laboratory of the Bureau of Standards and has long been in regular use there. Since thermoelements are so useful in determining temperatures the paper ought to interest both physicists and chemists.

Distribution of Energy in the Visible Spectrum of an Acetylene Flame.—It is important from a practical standpoint to know the quality of acetylene light—that is, the relative proportion of the different colours in the visible spectrum of the acetylene flame. It is also important to know the luminous efficiency—that is, the ratio of light to heat in an acetylene flame. The publication enclosed gives the numerical results of a research on the measurement of the energy distribution in the visible spectrum from the acetylene flame—data of value in the scientific study of light sources.

An Interlaboratory Phometric Comparison of Glass Screens and of Tungsten Lamps, Involving Colour Differences.—The national standard of light has been maintained by the Bureau of Standards since 1909 by means of four watts per candle carbon filament lamps. The paper transmitted herewith covers an investigation of the variations in results which might be expected in various laboratories. The agreement may be considered remarkable in view of the difficulties and the different characteristics of the observers and the wide difference in methods employed.

Further Experiments on the Volatilization of Platinum.—The results were obtained in an investigation on the quality of platinum utensils and consists of observations on change in weight of several platinum crucibles of various degrees of purity at 700, 1000 and 1200 Centigrade. These cover the ordinary ranges to which platinum crucibles are subjected and covers a temperature range especially favourable for this work. The summary and conclusions to this are given on page 373 of the publication. These are in addition to the results previously announced in this general investigation.

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The books belonging to the Society are lodged in the library of the Institution of Electrical Engineers, and can be consulted there any day between the hours of 10 a.m. and 6 p.m., and until 1 p.m. on Saturdays, also on any evening when the Society of Electrical Engineers or the Röntgen Society meets. Books cannot be removed from the library.

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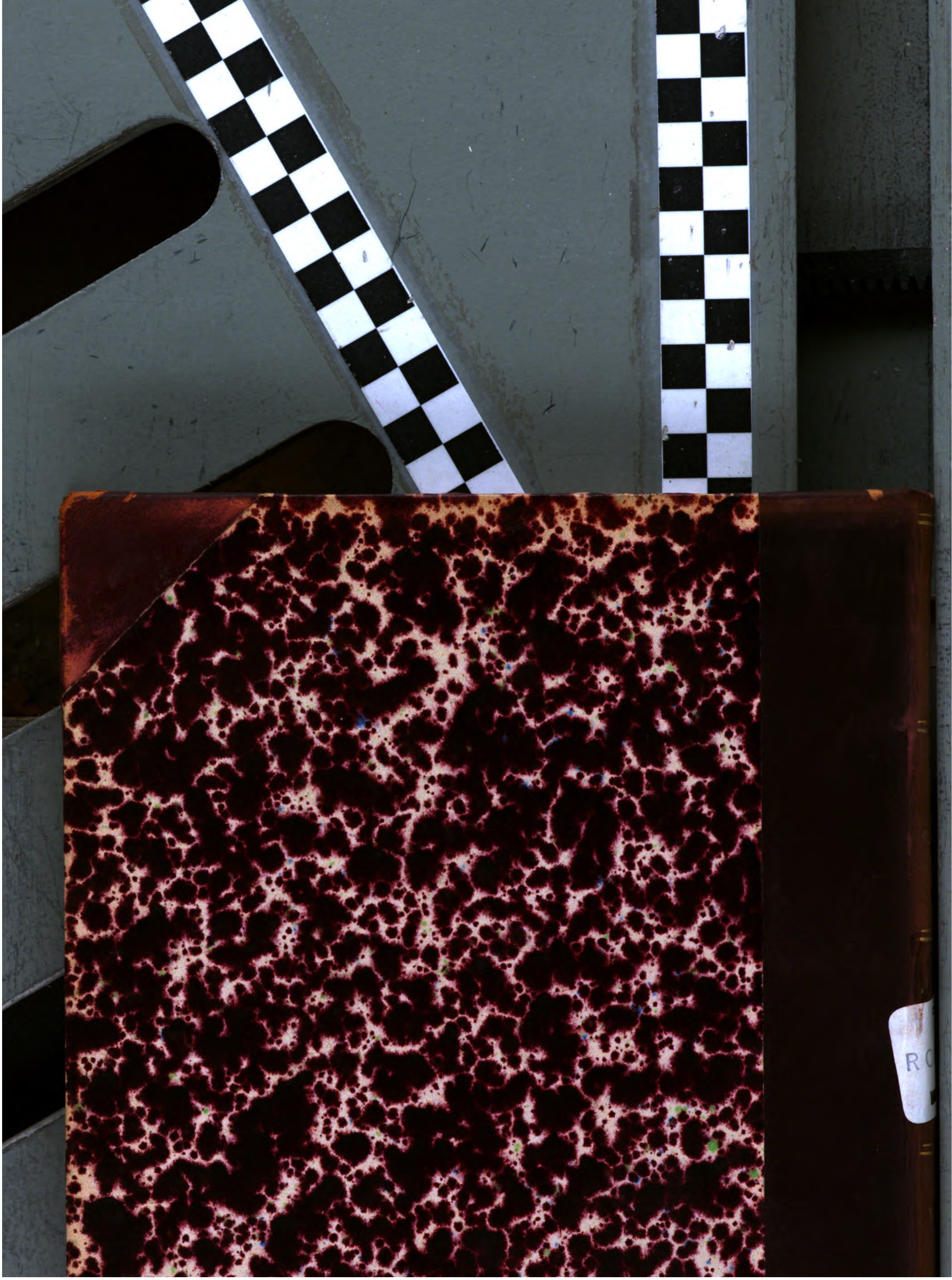

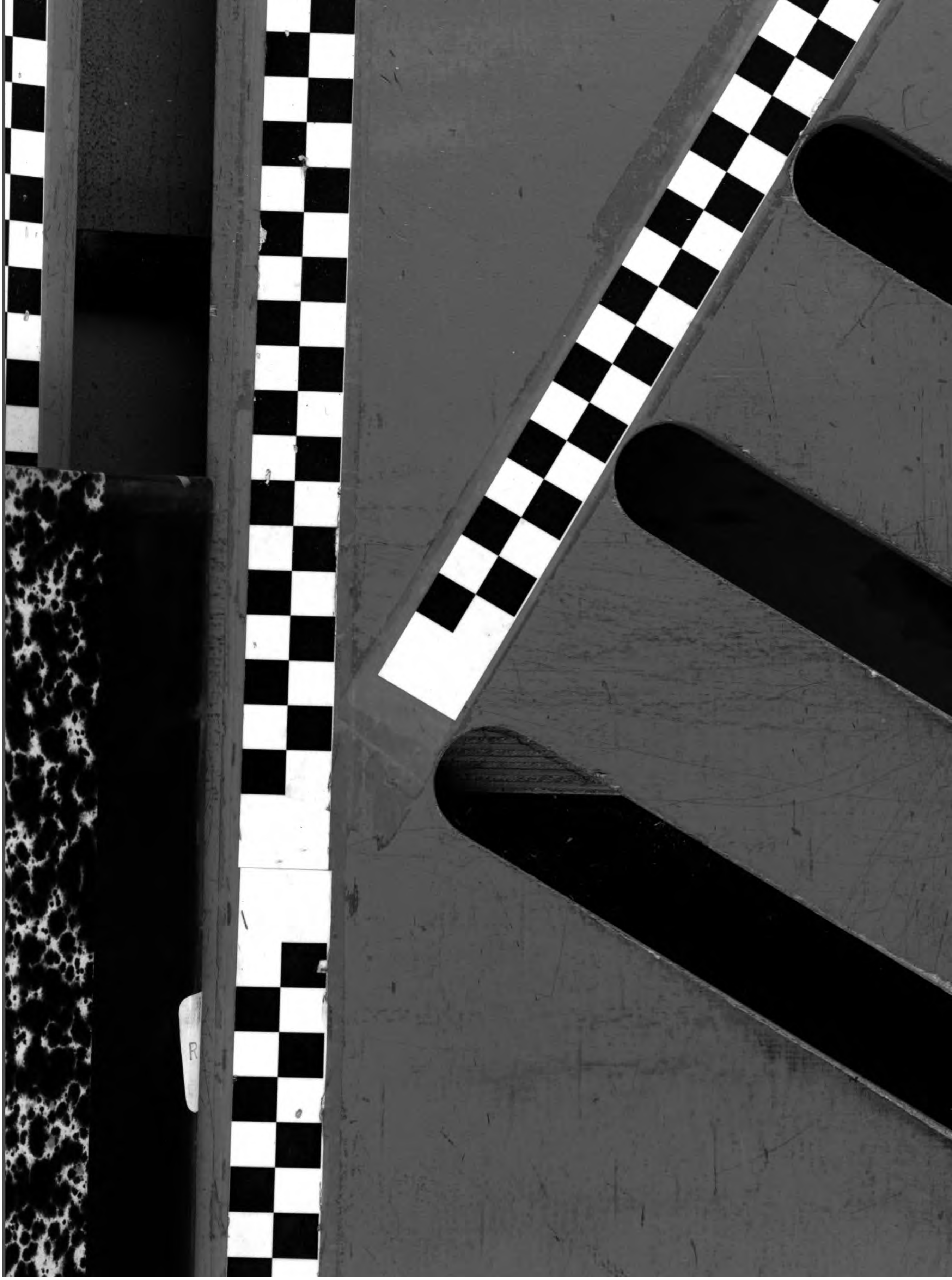
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